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Mr. Davy's new Electrochemical Apparatus.

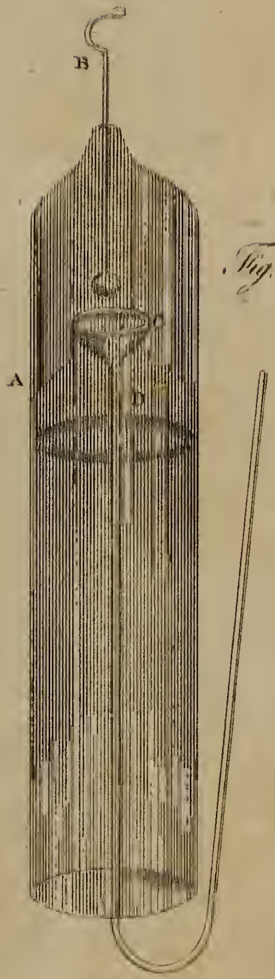
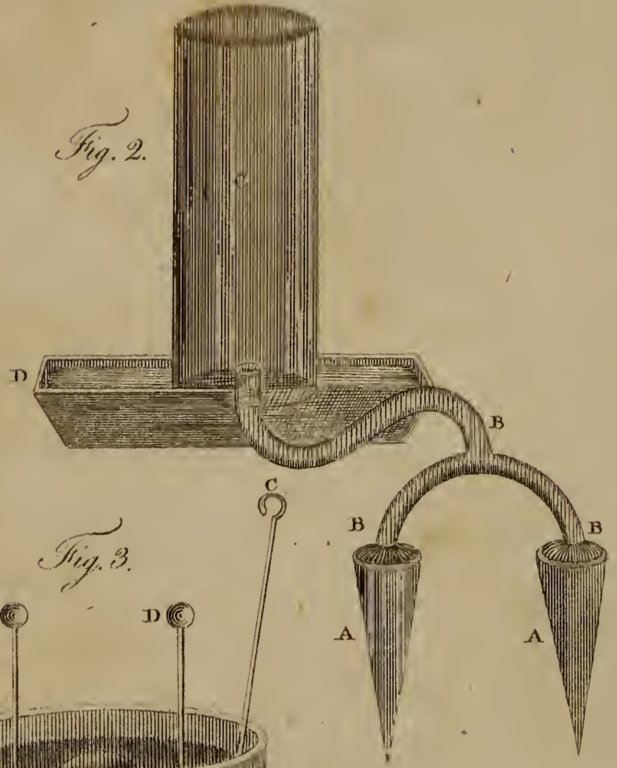


Fig. 1.



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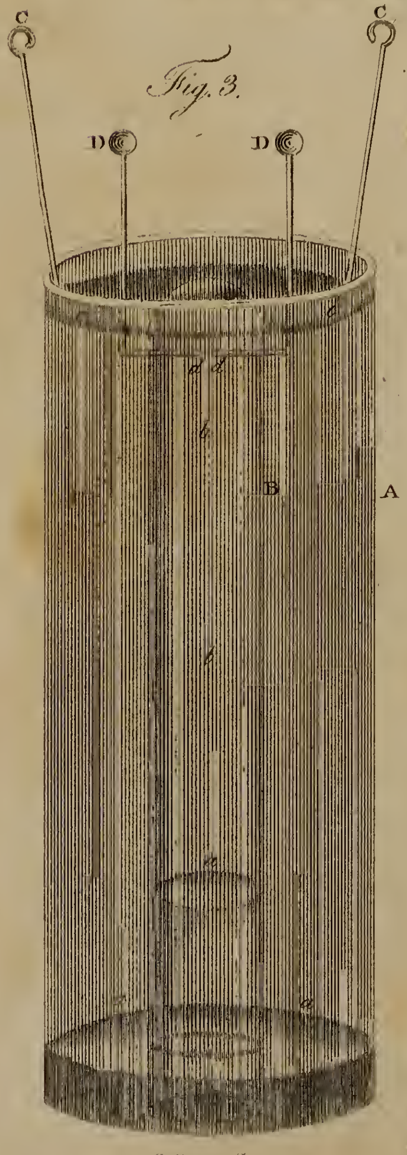

Fig. 3.

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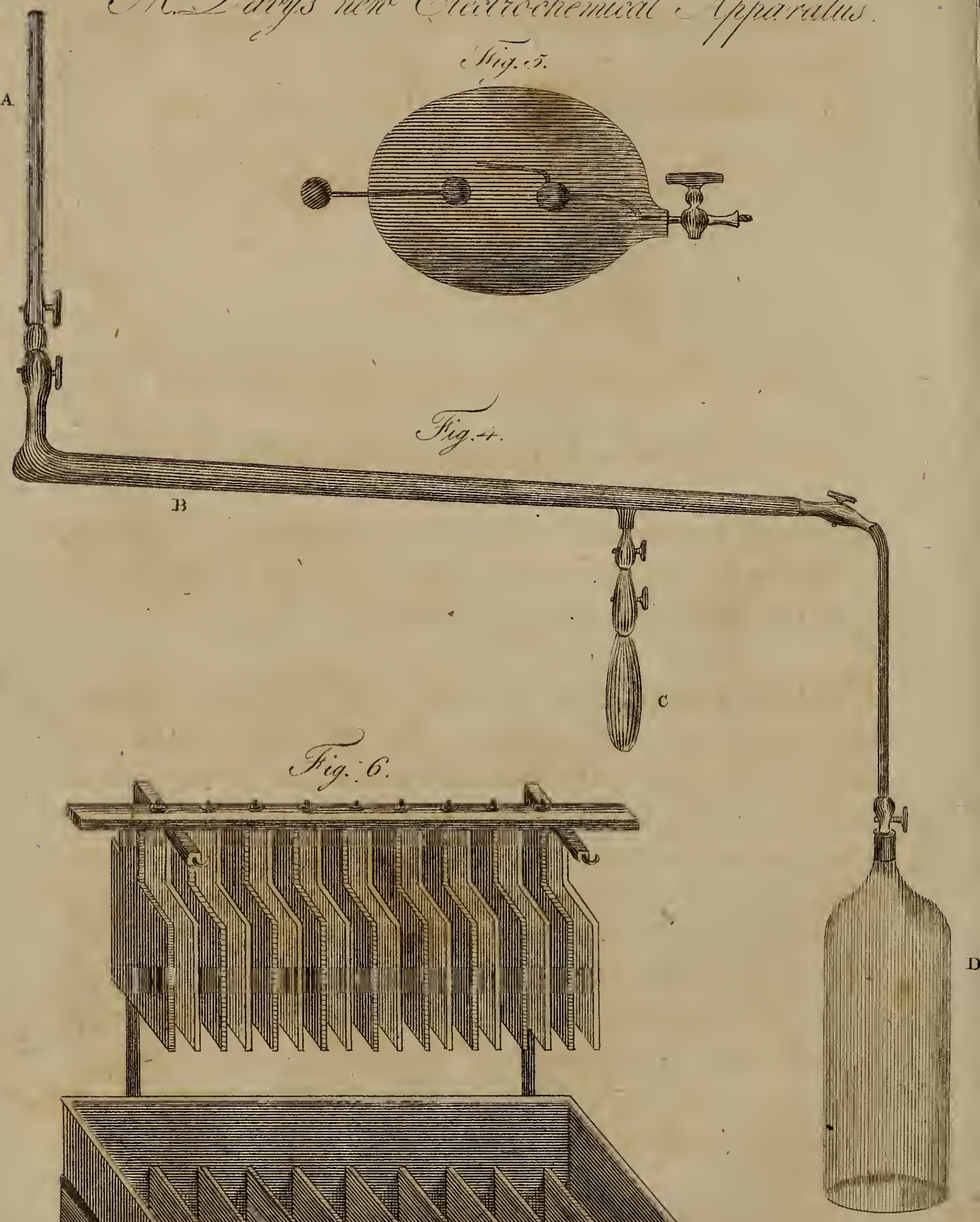
Mr. Davy's new Electrochemical Apparatus.



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MEDICAL AND PHILOSOPHICAL
Journal and Review.

Vol. III.—No. 1.

For the Medical and Philosophical Journal and Review.

A Case *of* Abscess *in the* Thigh, *in which the* Matter *made its way into the* Bladder. *Communicated to* Mr. Smith *by* Dr. David R. Arnell, *of Goshen.*

HIRAM Knapp, aged 18 years, of a tall, slender make, light complexion, and light eyes, but generally healthy, was taken on the 12th of October last, with a pain on the inside of the right thigh, directly under the s~~o~~artorius and vastus internus muscles: the pain was not so violent as to prevent him from labouring the first week after he began to complain, though so much so as to induce him to favour that leg, and bear the chief of his weight on the other; but the pain becoming more acute, at the end of the week he was so bad as to be confined to his bed: his whole complaint was the pain in his thigh, and sometimes in his heel. He was first attended by a Physician who supposed the pain to be rheumatic, and treated him accordingly.

I did not see him until the 13th of November, when I found him in most excruciating pain in his thigh, the pulse quick and low, the tongue furred with a thick, brown, dry crust; his countenance sunk; his knee raised in the bed, and could not be moved but with extreme pain; a discharge of blood in his urine, which he was obliged to make very frequently, though in small quantities, and without giving him any pain or uneasiness in the urinary passages; no external appearance of inflammation or tumour on any part of the thigh or hip, neither could I learn that any had been observed, or that an examination had ever been made of the hip-joint, as no suspicion had been entertained that his complaint lay there; nor could I discover any uneasiness in pressing any part of the hip, although I was particular in pressing on the part external to where the femoral artery passes over the brim of the pelvis.

After I had examined him, I informed his friends that I considered the pain in his thigh to arise from association with the hip-joint; that I considered his disease to lay entirely there; that the matter had by some means, which I could not explain to them, made a passage into the bladder, and was expelled with his water; that the matter being emptied in that way was the reason why I could discover no tumour or swelling in any part; and the parts being thus empty and flaccid was the reason why he did not experience any pain when pressure was applied to them; that his disease was connected with scrophula; that I did not think he could live long, and all that could be expected was to keep him as comfortable and easy as his unhappy situation would admit and it was in our power to do: with this view I ordered him some doses of opium, applied a blister to his thigh, and left him. I saw him again the second day after, and found him more comfortable and easy as to pain, though his disease was rapidly progressing; his leg and thigh could be stretched out, but I could not then discover that this leg was longer than the other; his water contained less blood, but in each discharge there was a quantity of sanious matter; his tongue was considerably dark coloured, his pulse more feeble, his appetite gone, and his strength failing; he continued very much in this situation until the 20th, when he died.

His friends living six miles from this place, I did not hear of his death until after he was buried; but considering this to be a new case in surgery, attended with symptoms of which I had never read nor heard, and wishing to know if my opinion was correct concerning the passage of the matter from the hip-joint into the bladder, I felt anxious to have an opportunity of dissecting him, for which purpose I obtained leave of his friends to raise him; which was done 60 hours after his death. The following were the appearances upon examination and dissection, viz. The whole length of the right leg and thigh was of an equal size with the left; on comparing the length of his legs, the right was about one inch longer than the other, the inside of the os femoris, from about two inches below the trochanter minor, and about four inches in length, and one inch in width, was perfectly white and bare, the periosteum and other integuments were separated from the bone, although no collection of matter was found there or below it: in this place was the whole complaint of pain: immediately below the insertion of the obturator externus and gluteus maximus was found a deep seated abscess, which contained about a pint of matter. From this abscess a passage was formed under the muscles, over the os pubis, and under the peritoneum into the bladder, near its neck; the capsular ligament on the outer side of the joint was sound and entire, the ligamentum teres and the cartilage which lines the acetabulum, and that which covers the head of the os femoris, were entirely destroyed and gone, the head of the os femoris, and the inside of the acetabulum were consequently rough and uneven.

It may be proper to remark, that in 1799 he complained of this thigh; that an abscess formed around the hip-joint, which I opened in his groin; that it discharged a large quantity of sanious matter, and that he was under my care for more than a year before it was cured, though after it healed he experienced no uneasiness, and the limb appeared perfectly sound until the time above mentioned. I do not, at this late period, remember the particular symptoms of disease which he then had, but this much I distinctly recollect, that I earnestly enjoined upon him rest; that I considered his disease as connected with scrophula, and treated it as such.

For the Medical and Philosophical Journal and Review.

ACCOUNT *of a* CASE *of* STRANGULATED HERNIA, *with the Appearances on Dissection.* *By* J. AUGUSTINE SMITH.

ON the 29th December, 1809, I was requested to attend a woman living in Mulberry-street, who was threatened, as I was informed, with a mortification in her side. Upon my arrival I found her with a high fever, and in great pain, in which situation I understood she had been for a week. Upon examining the side, I found that gangrene had already taken place, to the extent of an inch or an inch and a half, nearly over the right abdominal ring. I immediately suspected that she was labouring under Strangulated Hernia, but upon inquiry I was told that there had never been any perceptible swelling at that place; (none could at that time be observed) that during the whole course of her illness, the alvine discharges had been regular; that her stomach had not been at all affected, nor had pressure upon the abdomen ever given her any pain. Unable to account for the symptoms, I directed an opiate draught to relieve the pain, and some stimulant application to the part to promote a separation of the slough.

30. Much as yesterday; the mortified part shows some disposition to separate.

31. Last night a partial separation took place, when a discharge of flatus and an evacuation of fœcal matter explained the true nature of the disease.

January 2, 3, 4, 1810. The mortified parts are thrown off, but the ulcer looks foul, discharging ill conditioned pus and fœcal matter, which, from its appearance, I suppose comes from the ileum. Her pulse is moderate both as to frequency and force, but she will not take any kind of nourishment.

6. Yesterday she had a natural evacuation, and from this time forward no more fœcal matter was discharged from the groin. The pus secreted by the ulcer is fœtid in the extreme, ill digested, and dark coloured; nor does her health improve. She refuses every thing but a little drink.

8. Has had severe rigors and cold sweats; the discharge from the part increases.

From this time she continued to grow worse, but she lingered until the 16th, when she died.

Two hours after death I examined the body. The integuments of the abdomen were relaxed and much loaded with fat; upon cutting into the abdominal cavity, every thing at first appeared natural; no adhesions between the folds of the intestines and no trace of peritoneal inflammation. Two or three bands, evidently not of recent formation, connected the ascending colon to the internal surface of the abdomen. Upon further examination, the ileum was found opening into the colon in the usual manner, but there was not a vestige of the cœcum remaining, that part having been as exactly removed as if it had been done by the knife of an anatomist. The ulcerated edges of the intestine having been in contact, had united, which caused the fæces to resume their natural course. The protrusion had probably taken place under *Poupart's* ligament, for the destruction of the neighbouring parts was so extensive, that this could not be ascertained with certainty. The ulcer extended from near the linea alba, downwards a little way on the thigh, and upwards and backwards towards the superior and posterior part of the spine of the os ileum. The iliacus internus muscle was nearly destroyed. The discharge and irritation from this extensive ulcerated surface were the causes of the patient's death.

The above case is important in as much as it proves that mortification may take place in a portion of strangulated intestine, without the supervention of any of the symptoms which usually mark such an occurrence. Fortunately these cases are extremely rare, for they are unattended by any circumstances which can apprize the surgeon of the situation of his patient, until it is too late for his art to be of much service.

For the Medical and Philosophical Journal and Review.

HISTORY *of a* CASE *in which a large Quantity of* ARSENIC *was taken, with the Appearances on Dissection. By* J. AUGUSTINE SMITH.

A GROCER in this city, having determined to put an end to his existence, bought an ounce of arsenic, under a pretence that he wished to kill rats with it, and going home he mixed it in a glass of water, and swallowed the whole, except a very small portion, which adhered to the bottom of the glass.

From the accounts of all those who were with him, (for I did not see him until I went to examine the body after death) the effects of this enormous quantity of the poison were apparently much less violent than those which usually supervene from a few grains of it. He puked two or three times, and complained of some heat and pain in his stomach and bowels; but all who saw him, (his medical attendant among others, a gentleman of great experience in his profession,) declared that his sufferings were inconsiderable. As to his having taken the quantity above mentioned, there can be no doubt, as he himself confessed it, and told where he procured the arsenic, and the apothecary, upon application, confirmed the account.

In eighteen hours from the time he took the poison it proved fatal, and in about an hour and a half afterwards, I opened the body. The stomach and small intestines were distended with flatus, and but very slightly inflamed. No other morbid appearance could be discovered. It was thought unnecessary to examine the contents of the stomach, as he was seen to swallow the arsenic. He told his wife that he was about to take an emetic.

The effects produced by the arsenic in this case, differ very materially from those which usually follow the introduction of any considerable quantity of that mineral into the stomach. In the case related by Dr. Yelloly[*] the patient experienced an inconsiderable degree of pain, but the inflammation was greater than in the instance above related; in this, death appears to have been the consequence of some direct effect of the arsenic upon the nervous system.

For the Medical and Philosophical Journal and Review.

ACCOUNT *of a* TRIAL *made with the* WOOD ALCORNORQUE: *Communicated by Professor* MAC NEVEN.

THIS wood, which has been lately recommended from the West-Indies, as a specific in ulcers of the liver and lungs, is of a solid texture, ponderous, yellow colour, of a grateful bitterish taste, and sensibly stimulant.

The manner of employing it, according to the prescription of those whom it is reported to have cured, is to take twelve ounces of the wood, free of its coarser bark, and rasped or broken small. This quantity is then infused for twenty-four hours in three quarts of soft water, and the patient is to drink the whole infusion in the course of the day: the first tumbler to be taken warm in the morning with an addition of two spoonfuls of honey, and another at night, prepared in the same manner.

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[*] Vide Edinburgh Medical and Philosophical Journal, vol. v. p. 389.

During the period of its administration the patient is directed to abstain from milk, acids, spices, and all kinds of stimulant food; but to use asses milk after laying aside the remedy; and if a pain be felt in the side, to make a cataplasm of the powdered wood, and apply it to the part.

On the 3d or 4th day the patient generally experiences great anxiety; on the 5th or 6th an inclination to vomit, at which time also even blood is thrown up, and the matter of the ulcer. On the 9th and 10th he is better, coughs but little, and the following days he is convalescent.

Such are the encouraging relations sent with this new remedy to this country. Although I have not found it, in the trial I made of it, equal to this character; yet, as it possesses active powers, and has, it is to be presumed, been of service in a complaint, in which other remedies so frequently fail, every experiment on its virtues merits publication, until such time as the extent of its efficacy be well ascertained. The patient on whom I observed the effects of the Alcornorque is Mrs. D. of this city. She took it under the direction of Dr. Berger and myself, and after the manner already directed.

This lady has laboured under phthisis these two years past. She is of a lax fibre and phlegmatic temperament; her attacks of fever were never very violent, but of frequent recurrence; her cough has never quit her, from the first invasion of her disorder to the present time, and for more than a year past, she is seldom exempt from night sweats. The summer before last, after her symptoms had been alleviated, she went to the country by the direction of Dr. Morton and myself, and stayed for a few days in Orange county. She returned much the better for her journey, and felt that sailing on the North-River was of all other things what did her the greatest service. The benefit gained by travelling was soon lost by the sedentary life she led in town, yet she struggled through the winter better than might be expected, and with the return of fair weather was taken by her husband to Philadelphia. She had but crossed to Paulus-Hook, when she felt herself already better, and daily mended while she travelled. The marked advantage of change of place and exercise is very conspicuous in this case; but yet, though it is a remedy superior to every other we possess, it is unfortunately one that all consumptive patients, and women especially, cannot always employ. Notwithstanding the efficacy of riding and travelling therefore, as a cure for consumption, something to remove or to alleviate the sufferings of those whom their adverse circumstances condemn to a town residence, and stationary life, is a desideratum in physic, and one that should engage the researches of the faculty.

Under these circumstances we had recourse to the Alcornorque. When Mrs. D. began to use it, she had regular febrile exacerbations every day, with night sweats; she slept well at night, and coughed and expectorated largely in the morning; her appetite was upon the whole pretty good. For seven months there was no flow of the catamenia. After drinking the infusion of alcornorque three days the fever ceased, her rest became more disturbed, and her cough troublesome, probably because she remained more awake. The quantity of fluid drunk overpowered her stomach, and took away her appetite. Consequent upon this was an increased debility; yet she persevered for seventeen days, but could not possibly take the infusion any longer. She neither vomited, spit blood, nor experienced any extraordinary excitement. To her own feelings she seemed better in every respect but the debility, and was particularly gratified at the return of her catamenia, which appeared of a natural condition and quantity, after a few days use of the medicine. Since she left off taking it the state of her health does not differ materially from what it was before; but any difference that exists is rather for the better; and, upon the whole, so far as it is allowable to conclude from a single instance, this medicine appears to possess considerable activity, and to merit further investigation.

LETTER *on* CHIMNEY FIRE-PLACES, *addressed to* ROBERT R. LIVINGSTON, *L. L. D. President of the Society for the Promotion of Useful Arts. By* BENJAMIN DE WITT, M.D. *Professor of Chemistry in the University of the State of New-York.*

Albany, Dec. 10, 1810.

DEAR SIR,

I THINK I mentioned to you, last winter, that I had fitted up a fire-place in a very advantageous manner, by which much fuel was saved, and a great increase of heat produced. The return of the cold season brings this subject forcibly to my mind; and, where fuel is expensive, every improvement in this branch of domestic economy becomes of public importance. I propose then in this letter to give you a description of several fire-places constructed on the principle applied to mine; together with some observations on the usefulness and importance of that principle, when put into successful operation.

Whenever a fire is kindled in an ordinary open fire-place, the heat generated by the combustion of the fuel escapes in three different ways:

1st. A considerable quantity ascends the chimney in a state of *combination* with the air, vapour and smoke created by the fire. This is called *combined heat.*

2d. A large portion is absorbed by the hearth, back and sides of the fire-place, and is conveyed away through the walls by means of the *conducting* property of the materials composing it. This is called *conducted heat.*

3d. A part of the heat flies off in direct *rays* into the room, immediately from the fire, and also from the heated walls around it. This is *radiated heat.*

Now, it is evident that nearly all the heat passing off in the two first mentioned ways is actually lost, and does not contribute to warm the room. Count Rumford thinks it "quite certain that the quantity of heat which goes off *combined* with the smoke, vapour and heated air, is much more considerable, perhaps three or four times greater, than that which is sent off from the fire in *rays.*" And with respect to the *conducted heat,* I am confident that of this also a greater quantity passes away through the walls in contact with, and near the fire, than is thrown into the room by radiation. The truth of this observation will be sufficiently established, when it is found in the sequel that by the aid of this conducted heat, a room may be warmed with half the ordinary quantity of fuel. It appears then from these data, that but a very small proportion of the heat given out in a common fire-place comes into the room. The greatest part is dissipated without any advantage, and much fuel is consumed to no purpose.

Count Rumford, speaking of the radiated heat issuing from a fire, says, "Yet small as the quantity is of this radient heat, it is the only part of the heat generated in the combustion of fuel burnt in an open fire-place, which is, or *ever can be employed* in heating a room." Here, however, in the latter part of this remark, the Count evidently labours under a great mistake; for although the *latent* and *insensible* heat contained in the volatile materials ascending the chimney cannot easily be extricated and employed in warming a room, yet the *free* and *sensible* heat carried up by the vapour, smoke and air, may be brought back into the room, without difficulty, and made to contribute much to its warmth. In the Transactions of the Society for the Promotion of Agriculture, Arts and Manufactures of this State, published in 1799, there is an Essay on Warming Rooms, by Simeon De Witt, in which a method is described to accomplish this object. The *conducted* heat, also, which is usually carried away through the hearth and walls of a fire-place, may certainly be prevented from being lost. By a very simple contrivance, on the most obvious principle, it may be conveyed into the room, and employed in warming it with great profit. It is to the means of doing this that I wish particularly to invite your attention.

The burning of fuel in every fire-place, by heating and rarifying the air around it, always produces an ascending current; in consequence of which much air is constantly carried out of the room. Whatever quantity escapes in this way, the same quantity must come into the room in some other way, to supply its place. If the room is so tight that the external air can find no admittance into it, there will be no current ascending a chimney when a fire is kindled, and the whole apartment will be soon filled with smoke. This necessary quantity of air usually enters into rooms through apertures in the doors, windows and floors; by which means the air of such rooms, in cold weather, is cooled nearly as fast as it can be warmed by the fire. Now it is plain that if you can admit the requisite quantity of air through openings some where about the walls of your fire-places, where it will be heated before it is introduced into the room, and then make your doors and windows perfectly close, a double advantage will be gained. Much heat from the fire will be obtained, which otherwise would be lost, and the room will be constantly receiving warm air instead of cold; the consequence of which will be that the remotest corners of your apartment will be of a uniform and agreeable temperature. There will be a free circulation of fresh, but warm, air; and much less than the ordinary quantity of fuel will be sufficient to keep you comfortable in the coldest weather.

With these views, my brother, Mr. Simeon De Witt, whom I have already mentioned, had a close stove constructed, which he has used for two or three years past with great satisfaction. He closed his common fire-place by a wall built across it. Near this wall, and in front of it, he placed a small close stove, of a peculiar construction, and surrounded it with a white marble case, having an opening in the upper part of the front. The external cold air was admitted through a perforation in the hearth under the stove, passed up around it, within the marble casement, and issued warm into the room from the opening in front made for that purpose. The white marble, while it added to the beauty of the structure, protected the stove from the dust, which in other cases, by burning on the heated iron, gives a disagreeable smell to the air. He then made his doors and windows tight, and the effect was, that with a very small quantity of fuel, in the midst of winter, when the thermometer stood at 10° or 15° below 0°, he enjoyed, in his room, a fine fresh summer atmosphere.

But as close stoves are not so lively and pleasant as open fire-places, I undertook to apply this principle to a common Franklin stove; and accordingly procured a small one, cast with a flue projecting horizontally about four inches beyond the back. After closing my common fire-place by a straight wall, I placed this stove about two inches in front of it, and elevated about the same distance from the hearth. The flue entered of course about two inches into the wall. The opening around the edge of the hearth plate was then shut, so as to enclose the vacuity underneath, and the same was done as to the space behind the back of the stove. The external air was admitted through a hole previously made in the floor under the front semicircular part of the hearth plate. The current entering there spreads under the whole of the bottom, and behind the back, and issues into the room through openings for that purpose at the side of the stove. This air brings with it all the heat from the plates, which otherwise would be conducted away, and no cold air from without requires to be admitted. The room in which I have this small fire-place is large, with the doors of two smaller rooms opening into it, all of which are kept comfortable and pleasant, in the coldest weather, with less than half the ordinary quantity of fuel.

Many people, however, dislike even Franklin stoves; and it occurred to Doctor Charles D. Cooper, of Albany, that this principle might be applied without difficulty to fire-places of the ordinary shape and appearance; he accordingly had four plates cast, which, when put together, form a fire-place on the model of Count Rumford's. The hearth plate is let down on a level with the hearth stone in front of it. The ends of this plate and its front edge are supported on bricks, and there is a vacuity under the remainder of it, as well as behind the back and sides. From the upper edge of these plates the wall of the chimney is carried up, so as to close the opening above, and prevent the air behind the plates from going up the chimney. The cold external air is brought by a tube, four or five inches in diameter, from the nearest window, or any other aperture under the floor of the room, and is admitted through an opening under the front part of the hearth plate. This air spreads under that plate, and behind the back and side plates, and enters the room through horizontal perforations about five inches in diameter through each of the jambs, two or three feet above the floor. This fire-place does not differ in external appearance from a common Rumford, lined with cast iron plates.

But the manner of putting it up, so as to bring all the heat absorbed by the plates into the room, and, at the same time, to preclude the necessity of cold air being admitted by the doors and windows, gives it a great and decided preference. Doctor Cooper considered this fire-place so valuable and excellent that he has obtained a patent from the United States for its construction. His patent, I am informed, extends also to *Coal Grates* put up on the same principle. A coal fire, you know, usually creates a more intense heat at the wall behind it, than in front where the cold air enters. While the greatest part of the radient heat is intercepted, by the dead embers at the outside of the fire, the wall against which it burns will be red hot, and that heat is constantly conducted away, unless it is intercepted by some such means as I have described. Grates may, therefore, be constructed in this manner with peculiar profit and advantage.

The principle, however, on which the fire-places I have described are constructed, is by no means new; nor is the practical application of it to the warming of rooms original. I find that Doctor Franklin put it in practice sixty-five years ago. Yesterday, for the first time, I had the pleasure of reading the description of his Pennsylvania fire-place, published in 1745. To the back of the Franklin stove now in use, there was attached what he calls an *air box,* into which the cold air was received from without, and discharged warm into the room. The flame and smoke passed over this air box. descended behind it down to the hearth, and then ascended the chimney. He conjectured that ten barrels of heated air were by these means brought into his room every hour. If he was correct in this, no doubt his contrivance was successful; and I can only account for its not coming into general use, from the circumstance of its being too complicated in its structure, to be managed or understood by the common class of people. If these difficulties were the true cause of Doctor Franklin's original Pennsylvania fire-place being laid aside, I think they are obviated in a great measure by the simplicity of my method of putting up his open stove. It is attended with no more trouble, expense, or difficulty, than any other common method; and the benefits derived from it are incalculably greater.

In 1783 Doctor Franklin, in his letter to Dr. Ingenhousz, adverts to this principle as applicable to common fire-places, in the following words: "M. Gauger, a very ingenious and intelligent French writer, proposes, with judgment, to admit the air above the opening of the chimney; and to prevent inconvenience from its coldness, he directs its being made to pass in its entrance through winding cavities made behind the iron back and sides of the fire-place, and under the iron hearth plate; in which cavities it will be warmed and even heated, so as to contribute much, instead of cooling, to the warming of the room. This invention is excellent in itself, and may be used with advantage in building new houses; because the chimneys may be so disposed as to admit conveniently the cold air to enter such passages; but in houses built without such views, the chimneys are often so situated as not to afford that convenience, without great and expensive alterations." The difficulty here suggested by Doctor Franklin, seems to me to be altogether avoided in the method of putting up such fire-places proposed by Doctor Cooper. The plates may be adapted to almost every common fire-place, and all that is necessary in addition to this, is to make a perforation through each of the jambs, and one through the wall on which the hearth rests; from which last a tube is extended, commonly under the floor, to the most convenient place where it can reach the external air. Many of these fire-places have already been introduced into a variety of rooms in this city, at an expense comparatively small, and without any difficulty, in adapting them to every common fire-place. Two of them have been lately made in the Senate Chamber, by the commissioners superintending the building of the Capitol. And on the whole, considering the prevailing taste for open fire-places on the Rumford model, connected with the comfort to be derived from admitting warm, instead of cold air into rooms, and the great economy of fuel in consequence of that arrangement, I am persuaded that this fire-place is the best that has hitherto been contrived; and is well calculated for general use. The essence of the improvement made by Doctor Cooper consists in *the application of Franklin's principle to Rumford's fire-place, combining all the advantages of both,* which had never before been successfully accomplished.

Since the time of Doctor Franklin frequent and various attempts have been made to put his principle into operation. Fifteen or twenty years ago, I am informed, the late General Schuyler, of this place, endeavoured to apply it to a hall-stove in his house, and also to one in Mr. Stephen Van Rensselaer's house. Three or four years ago, in travelling through New-Jersey, I saw several fire-places of cast iron, said to be patent, erected with a view to derive the benefit of this principle. And with the same views Pollock's patent stove, and Gould's patent fire-place have been constructed. But the public have been disappointed in all these inventions. They have failed to produce the desired effect, from a radical error in the structure of the fire-places; and their error seems to have prevailed universally. The passages for the admission of air, in every instance, have been much too small to supply the requisite quantity of it. In some, the apertures for this purpose are not more than one inch in diameter; and they are nugatory in their effect. It is essential to the successful operation of fire-places of this kind, that there should be a due proportion between the openings for the introduction of air, and the opening or *choke* of the chimney by which it goes out of the room; for if the windows and doors are perfectly closed, as much air must necessarily come in at the former as escapes at the latter. If the room is tight, and the tubes for the admission of air are too small, but little heat, and little air will come into the room; the chimney will not draw sufficiently, nor will the fire burn well. If, on the contrary, the tubes are too large, the air will be admitted too freely. It will pass in before it is sufficiently heated, increase the draught of the chimney unnecessarily, and produce too great a circulation of the cold air. So far as I can judge from observation and experience, the circular *opening* for the admission of the *cold* and *condensed* air under the fire-place, should not be less than five inches in diameter; and the *heated* and *rarified* air should be admitted from thence into the room by at least two tubes of the same diameter. It is a good precaution always to make them rather too large than too small, for the quantity of air admitted into the room may then be regulated according to circumstances, by registers for that purpose.

I do not know that any one has ever put this method of warming rooms into complete and successful operation except Doctor Franklin, in his original Pennsylvania fire-place; Mr. Simeon De Witt, in his close marble fire-place; myself in the common open Franklin stove; and Dr. Cooper, in his cast iron Rumford. It is unaccountable to me, that such a plain, obvious, and important principle, should have been so much neglected in practice—A principle too, demonstrated by experience to be valuable and practicable, more than half a century ago. And, above all, it is a matter of astonishment that Count Rumford, in all his ingenious speculations, should have overlooked one of the most beneficial means of warming rooms, and neglected entirely the suggestions of the American philosopher. I am, Sir,

Respectfully, your most obedient servant,

B. DE WITT.

To R. R. LIVINGSTON, Esq.
President of the Society for the
Promotion of Useful Arts.

For the Medical and Philosophical Journal and Review.

An ACCOUNT *of* ZERAH COLBURN, *the wonderful* CALCULATOR: *Communicated by Professor* MAC NEVEN.

THIS child is a very extraordinary phenomenon, and must for ever reflect the celebrity connected with the birth-place of rare talents on the country of his nativity. He is not yet seven years old, and, in common, has all the playfulness and levity of his age. But when his attention is fully drawn to any subject, he exhibits an intelligence far superior to his years; when it is fixed on numbers, he peculiarly shines, and perhaps has then no equal of any age. His genius for calculation was first discovered last August, when his father overheard him muttering to himself the product of numbers, which he was adding or multiplying for his amusement. Since that time, through the notice which his wonderful powers have attracted, and their consequent exertion, the powers themselves have been greatly invigorated, and he now possesses a facility so prompt and so extensive, that he would seem to answer from memory, or to speak from intuition. The process of his computations is nevertheless a rapid combination only, for in more intricate calculations, he sometimes multiplies, adds, or subtracts aloud, and with amazing rapidity. He sometimes corrects himself; but much oftener corrects the slow and careful conclusions of those who come prepared to put questions to him. Whenever he does commit a mistake, he appears greatly mortified; but his accuracy is almost unfailing. I was present the other evening in a company where the following questions were put to him, and to all which he answered without half a moment's delay in any one instance, and without a single error.

Ques. What is the amount of 1347, 1953 and 2091?
Ans. 5391.

Ques. What two numbers multiplied together, produce 1242? He gave the following fractors as fast as he could speak them.

Ans. 54 and 23 3 and 414
9 — 138 6 — 207
27 — 46 2 — 621

Ques. What two numbers multiplied together produce 1044?

Ans. 18 and 58 3 and 348
6 — 174 2 — 522
36 — 29 4 — 261
9 — 116 12 — 87

Ques. What number multiplied by itself produces 1369?

Ans. 37.

Ques. What number multiplied by itself produces 2401?

Ans. 49; and 7 multiplied by 343 will produce the same.

Ques. What number multiplied by itself produces 5329?

Ans. 73.

When the questions were propounded with the thousands and hundreds by themselves; as, two thousand four hundred and one, or five thousand three hundred and twenty nine, the boy would impatiently cry out, put them in hundreds, that is, what number multiplied by itself will produce *fifty three hundred* and twenty-nine, &c.

Ques. What will 6 multiplied by itself 6 times produce?

This sum he worked aloud in the following manner, and as fast as he could repeat the words: 6 times 6 is 36; 6 times 36 is 216; 6 times 216 is 1296; 6 times 1296 is 7776; 6 times 7776 is 46656; and 6 times 46656 is 279936.

Ques. How much is 93 times 9143?

Ans. 850299.

Ques. What sum is produced by multiplying 117 by 77, deducting 63 from the product, and adding 40?

Ans. 8986.

Ques. How many times 37 in 45954?

Ans. 1242.

Ques. How many times 57 in 9999?

Ans. 175 and 24 over.

Ques. What is the number of hours in 7 years, 14 days, and 40 hours?

Ans. 61696.

Ques. How many hours in 25 years, 11 months and 3 days?

Ans. 226992.

The gentleman who prepared this question had committed a mistake in the product, so that when little Colburn gave the answer, he told him it was incorrect. The child, after an instant's reflection, insisted it was the gentleman himself was in error, and in going over the calculation again he found this to be the fact.

Ques. What will be produced by multiplying 123 by 237?

Ans. 29151.

The child's father objected to so difficult a question as three figures by three, but the boy himself said he could do it, and kept his word. In another instance he gave the product of 1234 multiplied by 1234, and did it with great promptness and accuracy. He was asked how many hours in 9125 days?

Ans. 219000 hours.

Ques. How many minutes in 219000 hours?

Ans. 13,140,000 minutes.

Ques. How many seconds in 25 years?

Ans. 788,400,000 seconds.

Nevertheless, very difficult questions fatigue him, and he frequently asks you not to give him such hard sums. While he is making out the answer, you perceive, by his countenance, how much his mind labours. His eye glistens, and his features contract. His physiognomy is very expressive; his forehead is rather small but angular, the orbital arch projects considerably; his eye is grey, intelligent, and always in motion. The cranium is arched, and remarkably broad between the parietal protuberances; the occiput is small; his hair is sandy. He is remarkably stout and well grown for his age. His movements are precipitate, and he is incessantly active.

He has never been at school, and neither reads nor writes. It cannot be said of him that his talents are merely premature, they are altogether extraordinary, and would be no less surprising at the age of five and twenty than at seven. On inquiring how he performed those calculations, he said he saw them quite plain before him. He has, as yet, no idea of fractions, but all his computations are confined to round numbers. He is the 5th of seven children, but his brothers and sisters are not distinguished from the mass of other children. The father, Abiah Colburn, has six fingers on each hand, and the same particularly distinguishes Zerah. He was born in Cabot, Caledonia County, State of Vermont, in April, 1804.

The general intelligence of Zerah Colburn distinguishes him advantageously from Jedediah Buxton, a person no less remarkable for his powers of calculation, but otherwise totally destitute of taste or talent. In music he could find nothing but a confusion of sounds, and while present at a play of Shakespeare's, performed by Garrick, he was intent only on the number of words, which were uttered by that great actor. Little Colburn is prompt at repartee, and sometimes sarcastic. Being asked, a few evenings since, by a lady who meant to be witty, how much was three naughts multiplied by three naughts—just as much as yourself, says he, —nothing at all.

If this uncommon gift of Zerah Colburn's should last, and be combined with even an ordinary degree of maturity of his other faculties, the mathematical sciences will probably be indebted to him for some abridged mode of calculation, not inferior to logarithms. But there is no analogy to countenance this hope. Jedediah Buxton had almost no other faculty but that of counting; and there is now living in this State, Mr. Van R. of the village of Utica, who was equally distinguished at the age of six, by a facility and correctness at calculation. At eight this faculty was lost as unaccountably as it had come, and ever since Mr. Van R. counts with pen in hand, no better or quicker than another, without having the faintest idea left of the manner in which he calculated from memory while a child.

For the Medical and Philosophical Journal and Review.

A Case, *exhibiting the Consequences of* Lues *cured by the* Rob *of* Laffacteur: *Communicated by Professor* Mac Neven.

Cases occur frequently in our practice, originating in venereal infection, and obstinately exhibiting many symptoms deemed venereal, but that far from being remedied by mercury, are under the use of every form of this mineral, rather exasperated than relieved. To mark the limits beyond which this valuable medicine should not be tried, is a matter of great practical importance, nor can these boundaries be better exhibited, perhaps, than by a detail of apposite cases. One of this kind came under my observation lately in this city, and I deem it of sufficient importance to be recorded, as at the same time, it underwent the scrutiny, and was subject throughout the treatment that cured it, to the inspection of persons every way qualified to judge of it with discrimination.

The gentleman to whom it relates was affected seven years since with chancres, for which, in the first instance, he treated himself. As usually happens under such management, he made bad worse, and then was forced to have recourse to persons of the faculty, under whose care he became apparently better. At last, however, the disease broke out again, and in the end he completely lost his health; he then consulted some eminent physicians, and was subjected by them to long and regular courses of mercury. These were not followed by any more permanent improvement of his condition. He lost his flesh, his appetite, and his spirits; he went from one watering place to another, and lost his money. He came to town, and was informed that his disorder was scrophula. He had constant nocturnal pains, for which he was forced to take continually increasing doses of opium, to the utter ruin of whatever little appetite or vigour remained to him. In this condition he desired to have my advice, in consultation with some others. On a thorough investigation of the case, we had no hesitation in pronouncing that the complaint was not scrophulous. The history of it afforded very strong presumption to the contrary, and this was increased by the actual state of the patient. The soft parts were not at all affected, but he had a long and elevated exostosis on the thigh bone, with excrutiating pain in that part, and in the bones of the forearm, when he warmed in bed. We were equally decided that no advantage could be reasonably expected from the further use of mercury, and, for myself, I was of opinion that strong decoctions of sarsaparilla and guiaicum, used in the manner I shall hereafter point out, would remove these remains of the venereal taint, and the abuse of mercury. At the same time another form of this remedy was proposed, with the efficacy of which I am acquainted. It is the *anti-syphilitic rob of Laffacteur.* It is a secret medicine, very much celebrated in France, and justly deserving all its celebrity. But Mr. *Allion*, a French chemist, gives the following as the correct receipt for preparing it. To this we are disposed to give credit, as it corresponds so nearly with a no less efficacious decoction employed in the French hospitals, and with what we ourselves know of the virtues of the same ingredients.

Composition of Laffacteur's anti-syphilitic rob.

Sarsaparilla, thirty ounces.

Marsh reed-grass (*arundo phragmitis*) thirty ounces.

Flowers of Borage, eight ounces.

Sena and roses, each two ounces.

Boil them in nine pounds of water for one hour, then strain the liquor; put an equal quantity of water on the residuum, and boil it again for nearly two hours; strain this, and to both decoctions add six pounds of sugar, as many of honey, and boil down the whole to the consistence of a rob.

The directions given with this medicine recommend it in those cases only of venereal infection, which have been aggravated by the abuse of mercury; they acknowledge that it is seldom serviceable in the primary stages of syphilis, and that it is quite useless in gonorrhœa.

There are many particulars insisted on, as to the preparation of the patient, and the mode of his taking the medicine; some of which are in themselves useless, though they may serve, in another respect, to increase his confidence and make him observant of those injunctions that are clearly beneficial. In order to exhibit a full view of this celebrated remedy, we shall give the directions at full length.

Preparation of the Patient.

1st day. He shall take two bottles of a light *tisane,* made from the leaves of borage, wild cichory, or even clean barley. He is to eat no ragaut, vegetables, or fruit, no wine, beer, cider, or coffee, but any other plain food he chooses.

2d day. If he be plethoric, a bleeding. Same tisane and diet as the day previous.

3d day. If his tongue and stomach be foul, and no pulmonary affection, an emetic.

4th day. He must take physic, and work it off with herb soup, or any light broth.

Administration of the Rob.

The day after the physic, he shall take the first dose of the rob, which for *men* is to be six table-spoonfuls, swallowed cold and without any addition, at six in the morning. For *women* it is to be four spoonfuls. Nevertheless, persons very much emaciated, or who have a slow fever, and those taking it in hot climates, are always to begin with a smaller quantity, say half.

Two hours and a half after the first dose of the rob, the patient shall drink a tumbler of tisane, cold in summer, tepid in winter, and made as follows.

Tisane.

Take of sliced sarsaparilla, two ounces in winter, one ounce and a half in summer; wash it and boil it slowly in three quarts of water to two, let it infuse all night, and strain it in the morning for drinking through the day. For the use of women and for warm climates, there may be a reduction of half an ounce of sarsaparilla. Six tumblers more of tisane are to be taken before dinner, at intervals of half an hour between each. If he began at six o'clock, he should dine at twelve. His dinner to consist of four ounces of bread, and about a fourth of his usual quantity of animal food, either mutton, beef, or fowl, roasted.

His drink to be the tisane, nor is any other allowable, even at his repasts, during the whole treatment.

The patient may eat fried or boiled fish, and fresh eggs, but milk in every shape is prohibited, even for a month after the treatment is concluded.

In four hours after dinner he is to recommence the rob, and take the same quantity as at first.

In two hours and a half more, he is to take a tumbler of tisane, and at intervals of half an hour four others, before supper. This is to be like his dinner.

Such is the uniform course to be pursued by the patient during the administration of the rob.

It is immaterial at what precise hour the patient begins, if he observe in taking his medicines, and between his meals, the intervals already marked. In winter, he may lie some hours in bed after the morning dose.

Four bottles of the rob are usually taken in succession, and then the use of it is suspended for four or five days. During this rest, the patient continues his tisane in the same quantity, and at precisely the same distance of time as while he takes the rob. His diet may be somewhat more full. On recommencing the rob, he must return strictly to the same regimen as at first, and if the case were so severe as to require six or seven bottles in all, the remainder are to be taken without any further interval.

Having finished the rob, the tisane is to be drank a fortnight longer, in the same quantity, and after the same manner, as during its use. The quantity of the aliments is to be increased as in a case of convalescence, but they must still be of the same kind, and strong liquors are not to be touched. The whole is to be concluded by a dose of physic.

It is proper, during the cure, to maintain the insensible perspiration, but without over-heating the room, which should not be raised to a greater temperature than from 70 to 80 of Fahrenheit. In the fine season the sick are not obliged to keep their rooms, but they should avoid damps and cold.

Some secretion is usually increased during the use of the rob, but even where there is no sensible increase of this kind, the cure is not the less certain. When the perspiration is too profuse, the tisane should be weakened by a third of water; costiveness should be alleviated by clysters of warm water, and if the patient be too weak to use animal food, his diet may consist of rice, soup, vermicelli, sago, and tapioca.

The patient, who is the subject of the account given here, conformed punctually to those directions, and was effectually cured. He is now plump and hearty, and in as good health as he ever enjoyed. By those who have much experience of the effects of sarsaparilla, a great deal will be ascribed to the virtues of that medicine alone, taken so largely as a diet drink, and much must belong to the uninterrupted use of it during so long a period, as well as to the care with which every thing is avoided that could interfere in any wise with the treatment.

In the French hospitals a medicine is usually employed for the same state of syphilis with that in which the rob is prescribed, and the advantages are scarcely less manifest or durable. This medicine is known under the name of the *syrup of Cusinier*, and is prepared as follows.

Sarsaparilla, ten pounds.
Sena, ten ounces.
Flowers of Borage, three ounces.
Flowers of Roses, three ounces.

Boil and strain, and add sugar and honey, each forty pounds, and reduce the whole to the consistence of a syrup.

The great point in the exhibition of these medicines is to give them in proper time and place. It would be worse than useless to exhibit them as substitutes for mercury in the primary stages of lues venerea. But it oftener happens that too much of that mineral is administered than too little, yet it is frequently in so desultory or injudicious a manner, as to create a mercurial disease, without eradicating the other. It is then that these vegetable remedies are so sovereign, and limited to this period, I have, from long experience, as much confidence in their efficacy, as in that of mercury itself for the first stages of syphilis.

What has always answered in my hands full as well as any thing I have seen or learned of the rob *anti-syphilitique*, is a saturated decoction of sarsaparilla and shavings of *guiai-*

cum, drank largely, observing at the same time, temperance and regularity in diet, and keeping the surface warm and perspirable. This is a point of great importance, if the season be cold. A warm climate should be formed in the patient's chamber, and as costiveness is apt to be produced by the increase of perspiration, the interposition of purgatives is indispensable.

The Bakerian Lecture for 1809. *On some new Electro-chemical Researches, on various Objects, particularly the Metalic Bodies from the Alkalis and Earths, and on some Combinations of Hidrogen. By* HUMPHRY DAVY, *Esq. Sec. R. S. F. R. S. E. M. I. R. A.*[*]

I. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries, on the application of electricity to chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views and applications not unconnected with the object of the Bakerian lecture: and though many of them are far from having attained that precision and distinctness which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

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[*] Phil. Trans. for 1810, p. 16.

Explanation of the Figures.

Plate I. Fig. 1. The apparatus for electrizing potassium in gasses. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water out of the contact of air. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a d c* the level of the different fluids.

Plate II. Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of £1000 has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected, from so great an accumulation of electrical power.

II. *Some new Experiments on the Metals from the fixed Alkalis.*

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metalic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expressed themselves satisfied both with the experiments and the conclusions drawn from them; but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquiries have given hypothetical explanations of the phenomena, different from those I adopted.

Messrs. Gay-Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hidrogen; a similar opinion seems to be entertained by Mr. Ritter. Mr. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hidrogen, with the alkalis; and an inquirer† in our own country regards them as composed of *oxigen* and *hidrogen.*

I shall examine such of these notions only as have been connected with experiments, and I shall not occupy the time of the Society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which Messrs. Gay-Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur; and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mem. d'Arcueil, a copy of which Mr. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail it seems, that they still retain their opinion; but upon precisely the same grounds as those, to which I have before referred. That no step of the discussion may be lost in the Society, I shall venture to state fully their method of operation, and of reasoning.

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* Journal de Physique, June, 1808; or Journal, vol. xxiv. p. 40.

† Nicholson's Journal, August, 1809, p. 258.

They say that the heated potassium* in ammonia, and they found, that a considerable quantity of ammonia was absorbed, and hydrogen produced; and that the potassium became converted into an olive coloured fusible substance. By heating this substance strongly, they obtained three fifths of the ammonia again, two fifths as ammonia, one fifth as hidrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two fifths, and found in the vessel, in which the operation was carried on, nothing but potash.—Again, it is stated, that, by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hidrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm, that they can procure from a given quantity of ammonia more than its volume of hidrogen.

Whence, they ask, can the hidrogen proceed?—shall it be admitted, that it is from the ammonia? but this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water, which may be supposed to be in the ammonia, or from the metal itself. But the experiments of Mr. Berthollet, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hidrogen gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hidrogen, and that alkali.

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* Mem. d'Arcueil, tom. ii. p. 309.

It is obvious, that, even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm, that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxigen which this would give to the potassium, and of the hidrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shown, by numerous experiments, many of which have been repeated before members of this Society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion, is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hidrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show, that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions, and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina, bored from a single piece, which, having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from 3 to 4 grains; and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation; and in these cases, there was a considerable deficiency of hidrogen, as well as nitrogen, in the results of the experiment. But when the heat was very slowly raised, the loss was much less considerable, and, in several cases, I obtained more than four fifths of the potassium, which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at 30·2 in.; thermometer at 54° Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7·5 were absorbed, and 3·2 of hidrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which with its adaptors, was exhausted, and filled with hidrogen. They contained together $\frac{8}{10}$ of a cubical inch. The heat was very slowly applied, by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxigen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed was such as to generate, by its action upon water, 3 cubical inches and $\frac{3}{10}$ of hidrogen gas.

Now, if this experiment be calculated upon, it will be found that 7·5—·2= to 7·3 of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hidrogen. But the 3·2 cubical inches of hidrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process = 9; and the nitrogen in the 8·8 cubical inches of gas, (or the 9—·2 of ammonia,) will be about 3; and if we estimate ·34 of hidrogen, and ·16 of nitrogen, in the 5 remaining in the retort, there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the $\frac{8}{10}$ of hidrogen pre-existing in the tube and adaptors, the loss of hidrogen will be found proportionably rather greater than that of nitrogen.

In another experiment, in which 3 grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hidrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors the same quantity of residual air, as in the process last described.

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia: and the unabsorbable part detonated with oxigen, in the proportion of 11 to 6, gave a residuum of 7·5. The barometer was at 30·2 in., thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6·78—·4 of ammonia = 6·38, and this quantity of gas, decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hidrogen. But there are produced in this experiment, of hidrogen 2·48 in the first operation, and 4·28 in the second; and, considering the nitrogen in the permanent gas as 3·32, 0·8 must be subtracted from the common air; which would give 2·52 for the nitrogen generated; and to these must be added the quantity of hidrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hidrogen.

In all experiments of this kind a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina. I exposed some of it to heat in a retort, containing oxigen gas; there was a diminution of the gas, and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from 3 to 4 grains of potassium were made to act upon ammonia in a vessel of platina, and afterward distilled in contact with platina, there were always from 4 to 6 grains of this powder formed; but I have advanced no farther in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxigen,

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hidrogen. When copper tubes are employed, the hidrogen bears a smaller proportion to the nitrogen; and more potassium is revived.

In those experiments, in which platina has been used, there is little or no loss of potassium or nitrogen: but a loss, greater or smaller, of hidrogen.

It will be asked, on what do these circumstances depend? Do the affinity of certain metals for potassium prevent it from gaining oxigen from ammonia? and do platina and copper combine with a small quantity of hidrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia it is not a *metallic* body that is decomposed, but the volatile alkali; and that the *hidrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia,* as I have always supposed; the potassium in the most refined experiments is *recovered,* but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxigen and hidrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

3·3 grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hidrogen; and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced equalled twelve cubical inches; and, by detonation with oxigen, proved to consist of nearly two of hidrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention the mere visible phenomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced, that it is the volatile alkali, and not the metal, which is decomposed in this process.

As sodium does not act so violently upon oxigen, as potassium, and as soda does not absorb water from the atmosphere with nearly so much rapidity as potash, sodium can be introduced into ammonia much freer from moisture than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green; it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form avidently by combining with one part of the elementary matter of ammonia, while another part is suffered to escape in the form of hidrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of Mr. Curaudau, that the metals of the alkalis are composed of the *alkalis* merely united to *charcoal.* The investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which Messrs. Gay-Lussac and Thenard have been misled by the moisture, which interfered with their results.

Mr. Curaudau states, that, when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in 8 cubical inches of oxigen: nearly two cubical inches of oxigen were absorbed, and soda in a state of extreme dryness, so that it could not be liquified by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phenomena, which I have described in the Bakerian lecture for 1807. Nearly 6 cubical inches of hidrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalis, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination, and it appears from Mr. Curaudau's method of decomposing the alkalis, that his metals must have been carburets not of potash and soda, but of potassium and sodium.

Mr. Ritter's argument in favour of potassium and sodium being compounds of hidrogen is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxigen than potassium, and, on the hypothesis of hidrogenation, must contain much more hidrogen; yet, though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,* sodium is heavier than potassium in the proportion of 9 to 7 at least.

On the theory which I have adopted, this circumstance is what ought to be expectad. Potassium has a much stronger affinity for oxigen than sodium, and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

Mr. Ritter has stated, that, of all the metallic substances he tried for producing potassium by negative voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that, when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxigen is given off at the positive surface; no hidrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives, that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hidrogen than that alkali.

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* Hassenfratz. Annal. de Chem. tom. xxviii. p. 11.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find, that, instead of proving that potassium is a compound of potash and hidrogen, they confirm the idea of its being as yet, like other metals, undecomposed.

When tellurium is made the positive surface in water, oxigen is given off. When it is made the negative surface, the voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hidrogen in water; which, in being diffused, is acted upon by the oxigen of the common air, dissolved in the water, and gradually loses a part of its hidrogen, and becomes a solid hydruret of tellurium. The compound of hidrogen and tellurium produced at the negative pole, when uncombined, is gaseous at common temperatures; and when muriatic acid, or sulphuric acid, is present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found at the beginning of this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which, when touched by water, did not inflame or effervesce, but rendered the water of a beautiful purple colour, and when thrown into water entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hidrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium; and that the *telluretted* hidrogen, (if the name may be used,) had formed with the oxidated potassium, *i. e.* the potash, a peculiar compound, soluble in water: and this I found to be the case; for, on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hidrogen; metallic tellurium was formed where it came into contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that, in the action of tellurium negatively electrified upon potash, potassium was produced, as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was farther confirmed by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hidrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hidrogen was evolved by the action of the alloy upon water; but the compound of telluretted hidrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other induced me to conceive, that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time by heated charcoal; and I soon proved, that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with twelve grains of well-burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hathett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found, that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aeriform* compound of tellurium and hidrogen; I shall mention merely some of its most remarkable qualities and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hidrogen is more analogous to sulphuretted hidrogen, than to any other body. The smell of the two substances is almost precisely the same.* Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but, after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalis. It precipitates most metallic solutions. It is instantly decomposed by oximuriatic acid, depositing a film, at first metallic; but which is soon converted into muriate of tellurium.†

As arsenic has an affinity for hidrogen, it occurred to me as probable, that it would present some phenomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface in water, by means of a part of the new battery containing 600 double plates, became dark coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

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* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, Esq. of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr Warburton co-operated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive, that *tellurium* might contain *sulphur,* not manifested in any other way but by the action of voltaic electricity, or of potassium; and some researches made upon the habitudes of different metallic sulphurets, at the voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hidrogen in the voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxigenation, could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and farther researches, made in the laboratory of the Royal Institution, proved, that the substance in question, was a new and singular combination.

† From the results of one experiment, which I tried, it seems that tellurium, merely by being heated strongly in dry hidrogen, enters into combination with it. An accident prevented me from ascertaining, whether the compound so formed is exactly the same as that described in the text.

Arsenic negatively electrified in a solution of potash likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark grey colour, and perfectly metallic; it gave off arseniuretted hidrogen by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic* were heated together in hidrogen gas, they combined with such violence, as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the voltaic battery.

As tellurium and arsenic both combine with hidrogen, it appeared to me probable, that, by the action of alloys of potassium with tellurium and arsenic upon ammonia, some new phenomena would be obtained, and probably still farther proofs of the decomposition of the volatile alkali, in this process, afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendour, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four sixths nitrogen, instead of being pure hidrogen, as in the case of the action of potassium alone.

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* In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash and white oxide of arsenic, Fourcroy, Connais. Chem. tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process, I find, that, though potash is decomposed in this operation, yet the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hidrogen. The gasses not absorbable by water, given off in this operation, are peculiar. Their smell is intensely fœtid. They are inflammable, and seem to contain charcoal, arsenic and hidrogen: whether they are mixtures of various gasses, or a single compound, I am not at present able to decide.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas, which was principally nitrogen, so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hidrogen, which are contradictory assumptions.

None of the chemists, who have speculated upon the *imaginary hidrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments, in which agents, which they did not suspect, were concerned. No person, I believe, has attempted to show, that, when potassium or sodium is burnt in oxigen gas, water is formed; or that water is generated, when potassium decomposes any of the acids:* and no one has been able to form potassium, by combining hidrogen with potash. I stated in the Bakerian lecture for 1807, that, when potassium and sodium were burnt in oxigen gas, *the pure* alkalis were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts af oxigen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed, yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find, that this is the case; for the results differ very little in some experiments, which I have made, upon considerable portions of potassium and sodium, procured by chemical decomposition.

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* When, in October, 1807, I obtained a dark coloured combustible substance from boracic acid, at the negative pole in the voltaic circuit, I concluded, that the acid was probably decomposed, according to the common law of electrical decomposition. In March, 1808, I made farther experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution, March 12. Soon after, I heated a small quantity of potassium in contact with dry boracic acid; no water was given off in the operation, and I obtained the same substance, as I had procured by electricity. Messrs. Gay-Lussac and Thenard have likewise operated upon boracic acid by potassium, and they conclude, that they have decomposed it; but this does not follow from their theory, unless they prove, that water is given off in the operation, or combined with the borate of potash; the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made hydruret of boracic acid.

When potassium is burnt in trays of platina, in oxigen gas that has been dried by ignited potash, the absorption of oxigen is about $\frac{11}{20}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.* The alkalis so formed are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalis, give indications of the presence of moisture.

Mr. d'Arcet has shown by some very well conducted inquiries, that potash and soda,† in their common state, contain a considerable proportion of water; and Mr. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in 100 parts 66·66 potash and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hidrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances I obtained from 10 parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash in muriatic acid gas. I shall give the results of two experiments made in this manner: 5 grains of potassium, inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, burnt with a beautiful red light,* and the whole mass appeared in igneous fusion; a little muriate of potash, in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about 5 of hidrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

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* The quantities of gas given out by the operation of water are in a similar ratio. See page 39 of this paper.

† Annales de Chimie. Nov. 1808, page 175.

The second experiment was conducted with still more attention to minuteness. 8 grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phenomena as in the last experiment, and the increase of weight of the tray was 6½ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium. where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed;

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* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Now, if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxigen, to form 9·4 grains of potash, and 6·6—1·4=5·2, the quantity of muriatic acid combined with the potash; which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash. But 35·6 of muriatic acid, according to Mr. Berthollet's estimation, would demand 71·1 of alkali, in the state of dryness in which it exists in muriate of potash; and 71·1—64·4=6·7; so that the potash taken as a standard by Mr. Berthollet contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali.*

After these illustrations, I trust the former opinions, which I have ventured to bring forward, concerning the metals of the fixed alkalis, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds,* than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hidrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water,* as an essential constituent of acids, alkalis, and oxides. This part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

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* Consequently Mr. Berthollet's fused potash must contain nearly 23 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

One of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia was, that nitrogen might possibly consist of oxigen and hidrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same path of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists on the origin of nitrogen, produced during the passage of water through red hot tubes, or the speculations of Girtanner, founded on these and other erroneous data: the early discovery of Priestley on the passage of gasses through red hot tubes of earthen ware, the accurate researches of Berthollet, and the experiments of Bouillon Lagrange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity. To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by voltaic electricity, so as to produce oxigen and hidrogen with great rapidity, and in which these gasses could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the voltaic combination were used, in such activity, that about the eighth of a cubical inch of the mixed gasses, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about half a cubic inch: it had been carefully purged of air by the air-pump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere while warm. After the first detonation of the oxigen and hidrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gasses; after every detonation this residuum was found to increase; and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, *i. e.* $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxigen; 6 measures mixed with three measures of oxigen diminished to 5; so that it consisted of 2·6 of hidrogen, and 3·4 of a gas, having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible, that, at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method, by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the preceding one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined; six measures of it detonated with three measures of oxigen, diminished to rather less than one measure. A result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hidrogen. That the hidrogen is in excess may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxigen, hidrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid, sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxigen and hidrogen, and is never produced from those two gasses alone. In the Bakerian lecture for 1806, I have stated several facts, which seemed to show, that the nitrous acid, which appears in many processes of the voltaic electrization of water, cannot be formed unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find, that the assertion, that both acids and alkalis may be produced from pure water, has again been repeated. The energy with which the large voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test, than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two cold cones in hidrogen gas, that no nitrous acid or alkali was formed. It might be said, that in this case the presence of hidrogen dissolved in water would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxigen gas, and the other in an apparatus, in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used; the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gasses were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases, for two or three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxigen, from oximuriate of potash, was any acid or alkali exhibited in the cones: even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though if the asbestus was touched with unwashed hands, or the smallest particle of neutrosaline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxigen and hidrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found, that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities, which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say: but as common salt perfectly dry is not decomposed by silex, it seems very likely, that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina, by voltaic electricity, in mixtures of the vapour of water and oxigen gas. I thought it possible, if water could be combined with *more oxigen,* that this heat, the most intense we are acquainted with, might produce the effect. When the oxigen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oximuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made red hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid however was generated: but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian Lecture, that, in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited, by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hidrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner, that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterward distilled from it; figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought into contact with the mixture of charcoal and potash when it was perfectly cool, and afterwards distilled from it by a slow heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of Mr. Berthollet, that nitrogen adheres very strongly to charcoal.* The circumstances, that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea, that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestley upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury while hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing, there was no notable increase of the volume. At the end of the experiment about $\frac{1}{50}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice, and thawed four successive times in a conical vessel of wrought iron. At the end of the fourth process the volume of the gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxigen, $\frac{3}{10}$ of hidrogen, and $\frac{6}{10}$ nitrogen.

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* Mem. d'Arcueil. tom. ii. p. 485.

Mr. Kirwan observed the fact, that, when nitrous gas and sulphuretted hidrogen are kept in contact for some time, there is a great diminution of volume; and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited, which has an ammoniacal smell. I repeated this experiment several times in 1800, with similar results, and I found, that the diminution of the volume of the gasses, when they were mixed in equal proportions, was rather less than ¼, which seemed to be nitrous oxide.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hidrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail toward the conclusion of this lecture, contains a volume of hidrogen equal to its own. But one of hidrogen demands half its volume of oxigen to convert it into water, and nitrous gas consists of about half a part in volume of oxigen; so that, supposing the whole of the hidrogen employed in absorbing oxigen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hidrogen over mercury, the barometer being at 29·5$^{\text{in.}}$, thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gasses quickly diminished; after two days the diminution ceased; and the volume became stationary; the barometer was at 30·45$^{\text{in.}}$, and thermometer 52^{c} Fahrenheit; when it equalled 2·3. The gas proved to be about ¾ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as

Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds: nitrogen, hidrogen, oxigen, and sulphur combining to form one; and a part of the nitrogen and oxigen becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompose nitrogen, by agents which I conceived might act at the same time on oxigen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed; but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of voltaic electricity.

I had an apparatus made by which the voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant; as soon as the contact with the potassium was made, there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxigen gas.

In all trials of this kind hidrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference, that nitrogen is decompounded in the process; but I found, that, in proportion as the potassium was introduced more free from a *cr~~a~~uft of potash*, which would furnish water, and, consequently, hidrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hidrogen, nearly equal to $\frac{1}{4}$ of the nitrogen, was produced. It cannot be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely, that the slight loss is owing to its combination with nascent hidrogen, and its being separated with the potassium in the form of the grey pyrophoric sublimate, which I have found is always produced, when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime, in its common state, is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hidrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hidrogen are readily decomposed by oximuriatic acid gas, a mixture of nitrogen and oximuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water; there was a small loss of nitrogen; but the greatest part came over densely clouded, and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenor of these inquiries cannot be considered as strengthening in any considerable degree the suspicion, which I formed of the decomposition of nitrogen, by the distillation of the olive coloured substance from potassium and ammonia in tubes of iron.

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming, that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned, yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxigen of which, in the beginning of the process of distillation, might form water with hidrogen, given off from the fusible substance; which, being condensed in the upper part of the tube, would be again brought into action toward the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hidrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer. Six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hidrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubical inch of ammonia, four of nitrogen, and eight of hidrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hidrogen, and 1·1 nitrogen. The potassium lost in the operation was sufficient to generate 3·1 cubical inches of hidrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft, it was not impossible, considering the recent experiments of Mr. Hassenfratz, that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which I conceive could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hidrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions, a loss of nitrogen, and a production of hidrogen were perceived in a case, in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha,* and it is possible that ammonia might have been regenerated by hidrogen from the naphtha, and absorbed by that fluid; and a part of the hidrogen might likewise proceed from the decomposition of the naphtha: and in several experiments, in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hidrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some farther operations, which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxigen was probably separated in this operation; but I was aware, that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxigen in this substance; at the same time it led to the suspicion, that of the two gasses separated by electricity, one, or perhaps both, might contain metallic matter united to oxigen: and the results of the distillation of the fusible substance from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper,) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock, which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture,* and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition was to produce the condensation of any aqueous vapour, which, if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gasses,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hidrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive coloured matter formed on the metal; but though in this case nearly eight cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to —15°, there was a very feeble indication given of the addition of hygrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hidrogen was to the nitrogen in the average proportions of from 73 or 74 to 27 or 26; the proper corrections being made and the precautions before referred to being taken.*

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* Philosophical Transactions, 1809, page 459. Mr. Berthollet, jun. in the second volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxigen, separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and at the same time he confutes some experiments, which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia.

Assuming the common estimations of the specific gravity of ammonia, of hidrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807, would be supported by these new experiments; but as the moisture and oxigen visibly separated cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of ammonia, I resolved to investigate more precisely, than I had reason to think had been hitherto done, the specific gravities of the gasses concerned in their dry state; and the very delicate balance belonging to the Royal Institution, placed the means of doing this in my power.

Nitrogen, hidrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be, at 30·5 in. barometer, 51° Fahrenheit's thermometer,

For nitrogen, the 100 cubical inches	29·8 grains.
For hidrogen, ditto	2·27
For ammonia	18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gasses evolved, there is a loss of $\frac{1}{18}$,* and if the smallest proportion be taken, the loss will be nearly $\frac{1}{12}$.

His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxigen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia, seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible, that the volume may be exactly doubled, and that the nitrogen may be to the hidrogen as one to three; but, neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

* 100 of ammonia, at the rate of 135, will give 136·9 of hidrogen, weighing 3·1 grains, and 48·1 of nitrogen, weighing 14·33 grains; but 18·4—17·4=1: and at the rate of 180, 133 of hidrogen weighing 30·1, and 47 of nitrogen, weighing 14; and 18·4—17=1·4.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible*, that the minute quantity of oxygen, which appears to be separated, is not accidental, but a result of the decomposition; and if hidrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hidrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hidrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices, with the hopes of being able to form it from ammonia in a dry state, but without success. Neither of the amalgams of potassium, sodium, or barium, produces it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of —20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hidrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hidrogen produced.

As it does not seem possible to obtain an amalgam in a uniform state as to adhering moisture, it is not easy to say what would be the exact ratio between the hidrogen and ammonia produced, if no more water was present, than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two: and in no instance, in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it will follow, that ammonia (supposing it to be an oxide,) must contain about 48 per cent. of oxigen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.*

If hidrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly of 48 oxigen, and 34 of basis; but if the opinion be adopted, that hidrogen and nitrogen are both oxides of the same metal, then the quantity of oxigen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis of the nature of metallic substances; but, if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hidrogen, and metallic by combining with a greater proportion.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter, as it is conceived to contain on the hypothesis of deoxigenation. In the last Bakerian lecture I have rated the proportion at

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* Even in common air, the amalgam evolves hidrogen and ammonia, nearly in these proportions; and in one experiment, which I lately tried, there seemed to be no absorption of oxigen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hidrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that, being in the nascent state, it would rapidly absorb oxigen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxigen had been absorbed, but this difference might have arisen, partly at least, from the mixture of hidrogen. Whether in any case the amalgam absorbs oxigen gas, is a question for farther investigation.

$\frac{1}{12000}$, but this is the least quantity that can be assumed, the mercury being supposed to give off only once and a half its volume of ammonia. If the proportion stated in page 53 be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1600}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully these ideas, and I shall conclude this section by stating, that, though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied to the manufacture of nitrous acid. And there is reason to believe, that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

IV. *On the Metals of Earths.*

I have tried a number of experiments, with the hopes of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electro-chemical processes on the alkalis, and the alkaline earths.

I find, that, when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine, or glucine, slightly moistened and placed in hidrogen gas; the iron becomes brittle and whiter, and affords by solution in acids an earth of the same kind, as that which has been employed in the experiment.

I have passed potassium in vapour through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour; so that there is great reason to believe, that the silex had been either entirely or partially deoxigenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

Potassium, in acting upon alumine and glucine, produced more hidrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable, that, even after ignition, water adheres to these earths.

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* The results of this experiment are opposed to the idea, that potassium is a compound of hidrogen and potash, or its basis; for, if so, it might be expected, that the hidrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found, that the potassium produced as much hidrogen, as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

The results of the action of the potassium were pyrophoric substances of a dark grey colour, which burnt, throwing off brilliant sparks,* and leaving behind alkali and earth; and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small, that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them, which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect, that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury while the tube was hot; but the alkaline metal gave the characters to the amalgam: and though, in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied, that there was any of the metals of these earths in triple combination.

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* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compound of potassium, sulphur, and charcoal, probably contains this substance likewise.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded by solution in acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the production of an amalgam from the metals of the common earths, yet I succeeded perfectly by the same method of operating in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder; and which, when acted upon by weak muriatic acid, gave off hidrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxigen which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

On a hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter; and that oxigen and inflammable bodies are in relations of attraction, which correspond to the functions of being negative and positive respectively; it would follow, that the attractions of acids for salifiable bases would be inversely as the quantity of oxigen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxigen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808 concluded, that barytes must contain the least oxigen of all the earths; and that the order, as to the quantity of inflammable matter, must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxigen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5* of metal per cent. strontites 86,† lime 73·5,* magnesia 66.‡

The same proportions would follow from an application of Mr. Dalton's ingenious supposition;§ that the proportion of oxigen is the same in all protoxides; and that the quantity of acid is the same in all neutral salts, *i. e.* that every neutral salt is composed of one particle of metal, one of oxigen, and one of acid.

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* Mr. James Thompson, and Berthier.

† Mr. Clayfield. Thompson's Chemistry, vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol. iii. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis being inversely as the quantity of oxigen contained by the basis, though gained from the comparison of the electrical relations of the earths with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalis and alkaline earths; which, in the case of the alkalis, were not very remote from what I had ascertained by direct experiments. Mr. Gay-Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxigen, might (as far as it is correct) be inferred from Mr. Dalton's law; though this ingenious chemist states, that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxigen, the same in all protoxides; and there is a proportion of acid, the same in all neutral salts; and new proportions of oxigen and of acid are always multiples of these proportions. So that, if a protoxide, in becoming a deutoxide, takes up more acid, it will be at least dou-

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition* of phosphate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

Mr. Berzelius, in a letter which I received from him a few months ago,† states, that in making an analysis of cast iron, he found that it contained the metal of silex; and that this metal, in being oxidated, took up nearly half its weight of oxigen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxigen, which agrees very nearly with the quantity of hidrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston; and though Lavoisier in-

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ble the quantity; and in these cases the oxigen will be strictly as the acid. Mr. Dalton's law even provides for cases, to which Mr. Gay-Lussac's will not apply; a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxisulphat of iron perfectly formed, (as some experiments which I have lately made seem to show,) there is probably only a single proportion of acid; and in the supertartrite of potash there is only a single quantity of oxigen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

* Thompson's Chemistry, vol. ii. p. 581.

† In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of Mr. Gay-Lussac, on the combinations of gaseous bodies, Mem. de Arcueil, tom. ii. p. 213; and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by Messrs. Gay-Lussac, and Thenard.

sisted upon this analogy with his usual sagacity; yet still the alkalis, earths, and oxides, have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls;* and the alkalis and alkaline earths are both distinguished by their solubility in water: but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalis and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and Mr. Guyton) precipitate the triple prussiate of potash: the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutrosaline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalis and earths with oils, to form soaps; and of the earthy soaps some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxigen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalis approach to the lightest class of oxidable metals?

It will be unnecessary, I trust, to pursue these analogies any farther; and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

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* Klaproth. Annales de Chimie, tom. x. p. 277.

It is probable that these alloys may be formed in many metallurgical operations; and that small quantities of them may influence materially the properties of the compound, in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold short iron; while, from another spot, it will make hot short: but by a combination of the two in due proportions, good iron is produced. May not this be owing to the circumstance of their containing different metals of the earths, which in compound alloys may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, Mr. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Hidrogen is the body which combines with the largest proportion of oxigen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would show, that it must be much more highly positive than any other substance; and, therefore, if it be an oxide, it is not likely that it should be deprived of oxigen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hidrogen are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shown, and as is farther proved by the researches of Messrs. Gay-Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decomposed, unless a new combination is formed: thus it is not changed by charcoal ignited in the gas by Voltaic electricity; but it is decomposed by all the metals; and in these cases hidrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxigen being found in the new compound. This at first view might be supposed in favour of the idea, that hidrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitromuriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxigenation.

That nitrogen is not a metal in the form of gas is almost demonstrated by the nature of the fusible substance from ammonia; and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches, that hidrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal; the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,* and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter to acid matter might be supposed capable of being ascertained by their relations to oxigen, or by the peculiar state of their electrical energy.

The whole tenour of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phenomena with at least equal facility.

If hidrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxigen, hidrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases with hidrogen; the earths, the fixed alkalis, metallic oxides, and the common acids, will be compounds of the same bases, with water.

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* The electrization of sulphur and phosphorus goes far to prove, that they contain combined hidrogen. From the phenomena of the action of potassium upon them in my first experiments, I conceived that they contained oxigen; though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxigen, till I discovered, that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hidrogen, and on phosphuretted hidrogen, I find that the phenomena differ very much, according to the circumstances of the experiment; and, in some instances, I have obtained a larger volume of gas from potassium, after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the Society. The idea of the existence of oxigen in sulphur and phosphorus is, however, still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium, I find, when heated in hidrogen, mixed with a small quantity of atmospheric air, absorb both oxigen and hidrogen, and become nonconducting inflammable bodies analogous to resinous and oily substances.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are, first, The properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hidrogen or oxigen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hidrogen, whether in its simple form, or in combination with one proportion of oxigen, or a double proportion, from my experiments, seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hidruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second. The facility with which metallic substances are revived, in cases in which hidrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of 6 inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed. The difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hidrogen is essential to the production of the metal.

Third. Oxigen and hidrogen are bodies, that, in all cases, seem to neutralize each other; and, therefore, in the products of combustion, it might be expected, that the natural energies of the bases would be most distinctly displayed, which is the case; and in oximuriatic acid, the acid energy seems to be blunted by oxigen, and is restored by the addition of hidrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hidrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable, that this is caused by the imperfection of the process:* and supposing potassium and sodium to produce the same quantity of hidrogen from ammonia and water, the circumstance at first view may be conceived favourable to the notion, that they contain hidrogen, which, under common circumstances of combination, will be repellent to matter of the same kind; but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for, on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then, whenever bodies known to contain hidrogen are decomposed by a metal, the quantities of hidrogen ought to be the same, or multiples of each other. Thus in the decomposition of ammonia by potassium and sodium, two of hidrogen and one of nitrogen remain in combination, and one of hidrogen is given off; and in the action of water on potassium to form potash, the same quantity of hidrogen ought to be expelled. From my analysis† of sulphuretted hidrogen, it would appear, that if potassium, in forming a combination with this substance, sets free hidrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett of the sulphuret of iron be made a basis of calculation, iron, in attracting sulphur from sulphuretted hidrogen, will liberate the same proportion of hidrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hidrogen, because in acting upon different combinations containing hidrogen they produce the evolution of equal proportions of this gas, then it might be proved, that almost any kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

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* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hidrogen evolved; i. e. whenever the metals of the alkalis act upon ammonia, supposing this body to be composed of three hidrogen, and one of nitrogen, in volume, two of hidrogen and one of nitrogen remain in combination, and one of hidrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalis and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

† The composition may be deduced from the experiments in the last Bakerian lecture, which show, that it contains a volume of hidrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hidrogen, and 32·73 of sulphur. When sulphuretted hidrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volnme, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phospuretted hydrogen, it was found, that when three gasses were decomposed by electricity there was no change in

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas evolve the same quantity of hidrogen, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common me-

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their volumes; but neither the arsenic nor the posphorus seemed to be thrown down in their common states; the phosphorus was dark coloured, and the arsenic appeared as a brown powder: both were probably hydrurets. This is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hidrogen: when the metal is in smaller quantity than is sufficient to decompose the whole of the gasses there is always an expansion of volume; so that arseniuretted and phosphuretted hidrogen contain in equal volumes more hidrogen than sulphuretted hidrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hidrogen, it would appear, that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

tallic oxides. If we assume the existence of water in the potash formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hidrogen in all inflammable bodies will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxigen, which may be supposed to exist in hidrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hidrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present occupy the time of the Society by entering any farther into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined to enable us to decide upon their nature, and their relations, or to form any general theory concerning them which is likely to be permanent.

REVIEW.

An Essay on the Causes of the variety of Complexion and Figure in the Human Species. To which are added, Animadversions on certain Remarks made on the first Edition of this Essay, by Charles White, in a series of Discourses delivered before the Literary and Philosophical Society of Manchester, in England. Also, Strictures on Lord Kaims' Discourse on the Original Diversity of Mankind. And an Appendix. By Samuel Stanhope Smith, *D. D. LL. D. President of the College of New-Jersey, and Member of the American Philosophical Society.* 8vo. pp. 411. New-Brunswick. J. Simpson & Co. 1810.

No work which has come before us since we commenced our critical labours evinces as much care and attention on the part of its author, as this essay. The style is nervous and dignified, the facts, particularly those which favour the author's hypothesis, are carefully collected, and judiciously arranged, and we certainly think Dr. Smith entitled to the credit of having made the most of his subject.

We were particularly pleased with his observations upon "the original and absolute savagism of mankind," and think he has clearly proved, that, as far as our knowledge and experience extend, such a state never existed.

"History," he observes, "presents to us no tribe originally and perfectly savage who has voluntarily sought from abroad, and introduced among themselves the manners and the arts of any civilized nation; much less has invented those arts, and cultivated those manners, from the operation of any causes arising solely within themselves, or any tendencies in human nature, while existing in such a state of society, towards further improvement. The unsuccessful efforts of the United States to introduce among the tribes of savages who skirt along our western frontiers only a few of our arts, most obviously tending to their own advantage, demonstrate that the genius of savagism is obstinately opposed to the labours, the restraints, and industrious habits required in civilized society. Hardly has any individual savage ever been induced to adopt our manners. Such, on the other hand, is the charm of their wandering and independent state, the pleasure of alternately pursuing their game, and reposing in indolence, that many of the citizens of the United States are found voluntarily to renounce all the conveniences of civilization to mingle with the savages in the wilderness, giving the preference to their idle and vagrant habits of life. Two striking and practical examples, which demonstrate, on one hand, with what facility civilized man sinks into the savage, especially in those circumstances which so frequently offered themselves to restless and idle spirits in the early periods of the world; and, on the other hand, what difficulties, almost insurmountable, the savage state opposes to the ascent of human nature, in the contrary progression towards the cultivation of the arts of civilized life.

"If such is the genius and character of savagism, as it appears in the aboriginal tribes of America, how much farther removed from the first elements of civilization must have been those primitive species of men contemplated by this hypothesis;

> Qui prorepsērunt primis animalia terris,
> Mutum et turpe pecus, glandem atque cubilia propter,
> Unguibus et pugnis, pugnantia?—
>
> Hor. Sat. lib. 1. Sat. iii. l. 98.

Compared with such beings the American Indian may be considered as an artist, and a sage. Compared with their hands, the only instruments afforded them by nature to dig into the earth for a miserable subsistence, the bow and the hook may be regarded as high and noble inventions. By such men, impelled by incessant and importunate wants; urged by the perpetual clamors of appetite; having their mental powers almost annihilated by the rudeness and miseries of their state, in which they could enjoy no leisure for meditation, no composure for reflection, no comparison of sentiment with others; and brutalized in all their faculties, their habits, and their tastes, it would have been impossible that one principle of science should have been discovered, or one liberal art ever have been invented. The existence of civilized society in the world is a proof that man was never in such an abject state. Infinitely more wretched than those animals which provide by instinct for their subsistence and accommodation, and are furnished with natural arms for the purpose, adapted to their respective states, a thousand ages would not have been sufficient to raise them to the art of the beaver.

"Besides, uncivilized man is a lazy, improvident, and filthy animal. If he has food for the present day, nothing is able to rouse him to industry. Contented, and even pleased with filth, because in that state he feels himself more perfectly at ease, for even the attentions of cleanliness are a constraint to a savage, he feels no motive to desire any accommodation beyond what he is compelled by necessity to seek. Men with such dispositions will be for ever stationary in whatever condition they may happen to be placed. Ages will elapse, as we have already seen in the North-American savages, and to them we may add all the independent tribes of the southern continent, without making a single effort to ameliorate their condition. Nothing but the controlling influence of some civilized power could ever induce a savage to wield a spade, or guide a plough. And all the ages of time would not be sufficient to teach him to separate from the ore, and to prepare the metal of which those instruments are made.

"A just philosophy, therefore, grounded on fact and experience, will lead us to the conclusion which the sacred scriptures propose as an elementary principle of our belief; that man, originally formed by a wise and beneficent Creator, was instructed by him in the duties, and the most necessary arts of life. Thus were laid, in the very commencement of the race, the foundations of domestic, social, and civil order. From the primitive man, thus instructed, have descended the various tribes of men upon the earth; and from him have been derived to his posterity, both the elements of religion which we perceive diffused through the original traditions of all nations, and the principles of the useful arts which we find cultivated among them from the earliest dawn of history."

"There is the highest reason to believe that the primitive man received from his Creator, along with his existence, such a knowledge of the qualities, powers, and uses of the various objects around him, together with such moral and religious principles, as would lay in his family, and among his immediate descendents, the true foundations of civilized society. Hence the primitive nations are, at their first appearence, in history, already civilized. Savagism was an after growth, which took its origin from idle, or disorderly men, who, abhorring the constraints of society, sought, in the bosom of boundless forests, that freedom from control, and from labour, which was congenial with their wandering disposition. By rapidly extending themselves over the uncultivated regions of Europe, and the North of Asia, they there prepared the elements of future nations. Thus mankind, either in a civilized or savage state, became diffused in time over the surface of the whole globe."*

The foregoing is, we think, a favourable specimen of

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* The following geographical remarks by Humboldt show how easy it was for this continent to receive its inhabitants from Asia. *Ed.*

"As it is more than probable that Asiatic and American tribes have crossed the ocean, it may be curious to examine the breadth of the arm of the sea which separates the two continents under the 65 degrees 50 minutes of north latitude. According to the most recent discoveries by the Russian navigators, America is nearest to Siberia on a line which crosses Beering's Straits in a direction from the south-east to the north-west, from Prince of Wales's Cape to Cape Tschoukotskoy. The distance between these two capes is 44 minutes, or 18 3-10 leagues of 25 to the degree. The Island of Imaglin is almost in the middle of the channel, being one-fifth nearer that Asiatic cape. However, it is not necessary for our conceiving that Asiatic tribes established on the table-land of Chinese Tartary should pass from the old to the new continent, to have recourse to a transmigration at such high latitudes. A chain of small islands in the vicinity of one another, stretches from Corea and Japan to the southern cape of the peninsula of Kamtschatka, between the 33 degree and the 51 degree of latitude. The great island of Tchoka, connected with the continent by an immense sand-bank, (under the 52 degree of latitude) facilitates communication between the mouths of l'Amour and the Kurile islands. Another archipelago of islands, by which the great basin of Beering is terminated on the south, advances from the peninsula of Alaska 400 leagues toward the west. The most western of the Aleutian islands is only 144 leagues distant from the eastern coast of Kamtschatka, and this distance is also divided into two nearly equal parts, the Beering and Mednoi islands, situated under the 55 degree of latitude. This rapid view sufficiently proves that Asiatic tribes might have gone by means of these islands from one continent to the other *without going higher on the continent of Asia than the parallel of* 55 *degrees*, without turning the sea of Ochotsk to the west, and without a passage of more than twenty-four or thirty-six hours. The north-west winds which, during a great part of the year blow in these latitudes, favour the navigation from Asia to America between the 50 degree and 60 degree of latitude. It is not wished in this note to establish new historical hypotheses, or to discuss those which have been hackneyed these forty years: we merely wish to afford exact notions as to the proximity of the two continents."

Dr. S.'s essay. We should now proceed to point out the errors and mistatements which it contains, but this we shall not do, first, because Dr. S. complains of undue severity in a former number of this work; to make him amends, therefore, we shall extend to him a lenity which he would not otherwise have experienced; next, we abstain from our wonted severity, lest peradventure our strictures should put the learned President into a passion, in which case we apprehend he would treat us as he has done* Mr. J. A. Smith, and fulminate the charge of *infidelity* against us, because, like Mr. S. we presumed to doubt the correctness of his hypothesis. The following paragraph will show what an opportunity Dr. S. has afforded us, were we disposed to take advantage of it.

" Before proceeding to treat directly of the causes of the various degrees of dark complexion observable in the different tribes of the human species, it will not be improper to propose two or three preliminary remarks on the structure of the skin, the seat of colour. This fine integument, although extremely delicate, and susceptible of the lightest impressions from many causes both external and internal, is, however, in its organic texture, among the least mutable parts of the human body. Hence any colour introduced into its substance is not easily eradicated. Figures stained in it with paints inserted by punctures become indelible. For the same reason freckles, though consisting only of partial stains impressed on the surface of a fair skin by a slight exposure to the sun and air, cannot be removed but with great difficulty;† and in persons of a certain ruddiness of complexion, such as is found commonly united with hair of a dark red, or deep orange colour, can never be entirely effaced."‡

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* No opinion has ever been expressed in this work which could militate directly or indirectly against revelation. The differences now observable among mankind, like many, very many questions in physiology, are at present inexplicable.

† " White may be regarded as the colourless state of skin, and all the shades of the dark colours as different stains inserted into its substance."

‡ " It has been remarked, and not without reason, that a dark colour of the skin may be considered as a universal freckle. And, certainly, if the same kind of secretion mingling with the perspirable matter issuing from the pores of the skin,

The following observations from Humboldt* contain some novel, and, as we think, conclusive information on this subject.

"The Indians of New Spain have a more swarthy complexion than the inhabitants of the warmest climates of South-America. This fact is so much the more remarkable, as in the race of Caucasus, which may be also called the European Arab race, the people of the south have not so fair a skin as those of the north. Though many of the Asiatic nations who inundated Europe in the sixth century had a very dark complexion, it appears, however, that the shades of colour observable among the white race are less owing to their origin or mixture than to the local influence of the climate. This influence appears to have almost no effect on the Americans and negroes. These races, in which there is an abundant deposition of carburetted hydrogen in the *corpus mucosum* or *reticulatum* of Malpighi, resist in a singular manner the impressions of the ambient air. The negroes of the mountains of Upper Guinea are not less black than those who live on the coast. There are, no doubt, tribes of colour by no means deep among the Indians of the new continent, whose complexion approaches to that of the Arabs or Moors. We found the people of the Rio Negro swarthier than those of the Lower Orinoco, and yet the banks of the first of these rivers enjoy a much cooler climate than the more northern regions. In the forests of Guiana, especially near the sources of the Orinoco, are several tribes of a white complexion, the Guicas, Guajaribs, and Arigues, of whom several robust individuals, exhibiting no symptom of the asthenical malady which characterizes *albinos*, have the appearance of true Mestizoes. Yet these

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which is fixed by the action of the sun or air, on certain points in freckles, should be equably diffused over the whole surface of the body, or throughout the whole substance of the skin, and we can discern no reason why it may not, every point would consequently be stained with the same colour.

* Humboldt's Political Essay on the Kingdom of Spain. New-York edit. p. 107.

tribes have never mingled with Europeans, and are surrounded with other tribes of dark brown hue. The Indians in the torrid zone who inhabit the most elevated plains of the Cordillera of the Andes, and those who under the 45° of south latitude live by fishing among the islands of the Archipelago of Chonos, have as coppery a complexion as those who, under a burning climate, cultivate bananas in the narrowest and deepest vallies of the equinoxial region. We must add, that the Indians of the mountains are clothed, and were so long before the conquest, while the aborigines who wander over the plains go quite naked, and are, consequently, always exposed to the perpendicular rays of the sun.* I could never observe that in the same individual those parts of the body which were covered were less dark than those in contact with a warm and humid air. We every where perceive that the colour of the American depends very little on the local position in which we see him. The Mexicans, as we have already observed, are more swarthy than the Indians of Quito and New Grenada, who inhabit a climate completely analogous; and we even see that the tribes dispersed to the north of the Rio Gila are less brown than those in the neighbourhood of the kingdom of Guatimala. This deep colour continues to the coast nearest to Asia. But under the 54° 10′ of north latitude, at Cloak-bay, in the midst of copper-coloured Indians with small long eyes, there is a tribe with large eyes, European features, and a skin less dark than that of our peasantry. All these facts tend to prove, that notwithstanding the variety of climates and elevations inhabited by the different races of men, nature never deviates from the model of which she made selection thousands of years ago.

" My observations on the innate colour of the aborigines differ in part from the assertions of Michikinakoua, the celebrated chief of the Miamis, called by the Anglo-Americans *Little Crook-back,* (Petite Tortue) who communicated so much valuable information to Mr. Volney. He asserted, "that the children of the Canada Indians were born as white as Europeans; that the adults are darkened by the sun, and the grease and the juices of herbs with which they rub their skin; and that that part of the waist of the females which is perpetually covered is always white."* I have never seen the Canada nations of which the chief of the Miamis speaks, but I can affirm that in Peru, Quito, on the coast of Caraccas, the banks of the Orinoco, and in Mexico, the children are never born white, and that the Indian caciques, who enjoy a certain degree of ease in their circumstances, and who remain clothed in the interior of their houses, have all the parts of their body, (with the exception of the hollow of their hand and the sole of their foot) of the same brownish-red or coppery colour."

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* The inferior part of the scrotum is, in persons of a dark complexion, several shades deeper coloured than any other part of the body, and yet it is of all others least exposed to the action of the sun. *Ed.*

"What we have been stating as to the exterior form of the indigenous Americans confirms the accounts of other travellers of the striking analogy between the Americans and the Mongol race. This analogy is particularly evident in the colour of the skin and hair, in the defective beard, high cheek bones, and in the direction of the eyes. We cannot refuse to admit that the human species does not contain races resembling one another more than the Americans, Mongols, Mantcheoux, and Maylays. But the resemblance of some features does not constitute an *identity* of race. If the hieroglyphical paintings and traditions of the inhabitants of Anahuac, collected by the first conquerors, appear to indicate that a swarm of wandering tribes spread from the north-west towards the south, we must not therefore conclude that all the Indians of the new continent are of Asiatic origin. In fact, osteology teaches us that the cranium of the American differs essentially from that of the Mongol: the former exhibits a facial line,[*] more inclined, though straighter, than that of the negro: and there is no race on the globe in which the frontal bone is more depressed backwards, or which has a less projecting forehead.[†] The cheek bones of the American are almost as prominent as those of the Mongol; but the contours are more rounded, and the angles not so sharp. The under jaw is larger than the negro's, and its branches are less dispersed than the Mongol's. The *occipital bone* is less curved, (*bombé*) and the protuberances which correspond to the *cerebellum,* to which the system of M. Gall attaches great importance, are scarcely sensible. Perhaps this race of copper-coloured men, comprehended under the general name of American Indians, is a mixture of Asiatic tribes and the aborigines of this vast continent; and it is not unlikely also that the figures with enormous aqueline noses, observed in the hieroglyphical Mexican paintings preserved at Vienna, Veletri, and Rome, as in my historical fragments, indicated the physiognomy of some races now extinct. The Canadian savages call themselves Metoktheniakes, born of the sun, without allowing themselves to be persuaded of the contrary by the *black robes,*[‡] a name which they give to the missionaries.

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* Volney, Tableau du Climat et du sol des Etats-Unis, vol. ii. p. 445.

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[*] "No name," it has been justly observed, "stands higher than that of Humboldt among the lovers of physical science," and yet he uses the term "*facial line.*"

[†] This extraordinary flatness is to be found among nations to whom the means of producing artificial deformity are totally unknown, as is proved by the crania of Mexican Indians, Peruvians, and Atures, brought over by M. Bonpland and myself, of which several were deposited in the Museum of Natural History at Paris. I am inclined to believe that the barbarous custom which prevails among several hordes of pressing the heads of children between two boards had its origin in the idea that beauty consists in such a form of the frontal bone as to characterize the race in a decided manner. The negroes give the preference to the thickest and most prominent lips; the Calmucks to turned up noses; and the Greeks, in the statues of heroes, have raised the facial line from 85 to 100 degrees beyond nature. (Cuvier, Anat. Comparée, t. ii. p. 6.) The Aztecs, who never disfigure the heads of their children, represent their principal divinities, as their hieroglyphical manuscripts prove, with a head much more flattened than any I have ever seen among the Caribs.

[‡] Volney, t. ii. p. 438.

The Modern Practice of Physic, exhibiting the Characters, Causes, Symptoms, Prognostic, Morbid Appearances, and improved Method of Treating the Diseases of all Climates. By Robert Thomas, *M. D. of Salisbury. From the third London Edition, corrected, and considerably enlarged. With an Appendix, by* Edward Miller, *M. D. Professor of the Practice of Physic in the University of New-York.* 8vo. pp. 697. Collins & Co. 1811.

An American edition has just issued from the press of Collins & Co. of this useful work, and the value of it has been greatly enhanced by the addition of a very able and conclusive essay on Yellow Fever, by Dr. Edward Miller. To a review of the latter, as the more novel part of the publication, we mean to confine ourselves; and yet, much of this same was already communicated to the public in the report which the Resident Physician addressed to the Governor of the State, in 1805. We recollect how satisfactory that account of the preceding epidemy appeared at the time to every dispassionate judge. Nevertheless, in the more methodized form it assumes in this essay, and with the additional matter it contains, it possesses higher claims to attention; and these, we apprehend, it will in no respect disappoint. Dr. Miller considers; 1st. What are the causes and nature of yellow fever. 2d. If it be a modern disease. 3d. If it be a contagious disease. 4th. If it be imported and exported. 5th. If it be found in the interior parts of the country. 6th. What are the relations and differences of yellow fever and typhus. Under these heads the most material circumstances appertaining to the disease are arranged in a lucid and satisfactory manner. Dr. M. considers the primary and essential cause of yellow fever to be a pernicious exhalation floating in the atmosphere. This is exhaled *by heat* from low and moist grounds, overspread with the corrupting offals of animal and vegetable substances, collected in large masses; or from any place where the process of putrefaction is going on to a considerable extent. The presence of a certain degree of heat is very properly exhibited as an essential condition. Where the disease is endemic, there is, without exception, a tropical degree of heat, and in those places occasionally visited by its ravages, it never appears if the thermometer has not stood for a considerable part of the summer at or about 80° of Fahrenheit. This is not all, but the low and marshy situations where the disease originates must be in a situation favourable to putrefaction, or no epidemy arises; thus, if the heat be so excessive as speedily to dry up the marsh, or the rains so abundant as entirely to cover it, there is no epidemy. Hence, it is most prevalent on the sea shore, for here, whenever heat and the materials of putrefaction combine, the necessary humidity is always present.

As the materials of putrefaction and the degrees of heat in a large city greatly exceed what is found in the country, so must the malignancy of the diseases arising in the latter be greater and more frequent; and that which would be remittent fever in the country will be yellow fever in town. So again, as the secondary or exciting causes, such as exposure to heat, fatigue, cold, dampness, intemperance, fear, anxiety, &c. may prevail among some members of the same family more than others, we shall find there the malignant forms of yellow fever, and the mild forms of remittent. These circumstances are justly insisted on to prove the identity of the causes of these forms of fever.

Is the yellow fever a modern disease? A reference to the works of the ancients will prove that it is not. A description of its most striking peculiarities will be found in the writings of Hippocrates, who practised physic on the shores of the Mediterranean, and in nearly the same parallels of latitude with those districts of America where this disease is most prevalent. The Commentaries of Galen are still more full on the subject, and where an easy reference may not be had to those works, the most material passages from them will be found in the more popular volumes of Van Swieten.

Is yellow fever a contagious disease? The whole section relating to this question is highly deserving of the reader's attention: we shall select from it the following very important facts.

> "It appears from the records of the yellow fever of 1805, in the city of New-York, that there were thirty-one streets of the city, most of which continued to be filled with inhabitants, through the whole season, in which only a single case in each occurred; and in the mass of six hundred cases, reported to the Board of Health in that season, there were only thirty-five houses in which more than a single case was found. If the number of deaths should be supposed to afford better ground of calculation, it will be found that there were forty streets, and those generally crowded throughout the season, in which only one death in each took place: not more than three died in any one house, of which there were only two instances; and, during the whole epidemic, there were only twelve instances of two persons dying in one house.
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> "From these reports to the Board of Health, it results, that upwards of five hundred, out of six hundred cases of malignant fever which occurred, were single in the respective families; and that more than three-fourths of the deaths which took place in the city were likewise single in the respective families in which they occurred."

How is it possible to reconcile such facts with the supposed contagiousness of yellow fever? With the non-contagiousness of yellow fever falls, of course, the possibility of its importation from abroad. But the author justly remarks that it may be *generated* in a foul ship, as well as in a filthy dock. We have seen this disease prevail lately in the village of Brooklyn, without its ever crossing the river to New-

York; and certainly, after such an example, we can give no credit to its being wafted to us across the atlantic.

Perspicuity requires that a clear distinction should be established between miasma and contagion. We find that this is accordingly done in the following passage.

> "By contagion is understood a noxious matter, produced by organic action of diseased human bodies, emitted from such bodies, or from substances which have been in contact with them, and causing a similar disease in persons to whom it is applied. Of such contagion the small-pox presents the most unequivocal example. By miasma is meant that noxious exhalation which arises from dead animal and vegetable substances, or either of them, undergoing decomposition, and which is the spontaneous result of attractions and repulsions conferred by nature on the elementary particles of which it is composed. Contagion, therefore, is a poison of animal production, and miasma a poison of chemical production."

Thus, by miasmatic poisons, should be understood those that are of chemical origin, and formed without any febrile, morbid, or organic action of any kind. By contagious, those that are of animal origin, and secreted by the vascular energy of the animal body.

The Doctor has proved, to our satisfaction, that yellow fever is a disease of domestic origin, and not contagious: nor can any thing more satisfactory than this publication be offered, as we apprehend, in the present state of our knowledge on the subject.

Highly as we approve of the first five sections of this essay, we must differ from the conclusions of the sixth and last. We are as convinced that typhus fever is communicable from one person to another, as that yellow fever is not. In the same way that a person breathing the marsh miasma at its source, and in a state of concentration, is likely to be infected by its poisons, so, if he breathe the exhalation issuing from the human body, and rendered virulent by the accessories of filth and heat, he is likely to be affected by typhus. If, then, the term *contagious* be understood of those diseases that originate in the human frame, and are taken by one person from another, contagiousness must be predicated of typhus, and after this manner we understand it.

A View of the Diseases most Prevalent in the United States of America, at different Seasons of the Year, with an Account of the most approved Method of Treating them. Being an Abstract, not only of the Editor's own Experience, but of the Experience of several Physicians of distinguished Abilities residing in the different States. Including the Substance of all the latest and most important Improvements that have been made in the Treatment of similar Diseases in other Countries. Collected and arranged by WILLIAM CURRIE, *Fellow of the College of Physicians of Philadelphia, Member of the American Philosophical Society, Physician to the Magdalen Asylum, &c.* J. & A. Y. Humphreys. 8vo. pp. 240. Philadelphia. 1811.

OUR curiosity was a good deal excited by the title of this book, and we expected, from such an implied promise, much instructive and original matter. If any diseases be peculiar to this new country, it is highly important they should be known; or if those already familiar to the physician possess, in this western hemisphere, any essential differences to distinguish them from the diseases of the same denomination on the other side of the atlantic, it is of equal moment to collect the points of difference, and investigate the nature of the modification. On taking up Dr. Currie's book we were, consequently, prepared to examine the endemical diseases of America, and the distinctive peculiarities of those that are common to it and Europe.

With regard to the first part of our inquiry, the first page of the introduction showed us that our task would neither be long nor tedious. We are told, that the maladies most prevalent "in the range of country included within the limits of the United States" are catarrh; pleurisy and peripneumony: hepatitis; cynanche tonsillaris; cynanche trachealis; gastritis and enteritis; rheumatism; arthritis; erysipelas; epistaxis; hæmoptysis and phthisis pulmonalis; the cholera and diarrhœa from the middle of July to the middle of September; the dysentery from the beginning of August to the middle of September; the intermittent, remittent, or bilious fever from the middle of August to the end of October; the malignant yellow or pestilential fever, the cynanche scarlatina, "which does not appear every year," &c. The English names of these disorders are very considerately annexed, for the information of those practitioners of physic who do not understand a little Latin, and we are sorry that the precaution is not more unnecessary.

Now, it so happens that we have seen a little of the practice of physic in Europe, and read some of the books that have been published there in different ages, even those that are written in Latin, and we could not but marvel how very like every thing to be found in the medical treatises of that part of the world is to what we find in Dr. Currie's learned publication. They have catarrh there in the beginning of winter as well as we have it here, and it is known by an increased secretion of mucus from the glands of the mucous membrane which lines the nostrils, &c. as any body may convince himself by looking into Dr. Cullen. The gout likewise gives terrible twitchings in the toe, and the rheumatism is prevalent in the spring, as Dr. Currie tells us it is in the middle States, for which see Dr. Cullen again; it also frequently occurs in the winter season in both hemispheres. A similar coincidence will be observed throughout Dr. Currie's book and Dr. Cullen's; not but our countryman coincides now and then in his descriptions with some of the other writers of Europe, or rather the countries themselves coincide in their diseases, and then he may be an original observer as well on this as they on the other side of the water. The treatment recommended throughout by Dr. Currie is very correct; we heartily subscribe to it, and, indeed, we deemed it judicious many years before he made this book. We do not see, if the people of Europe gain so much by clothing us in their manufactures, why the patriotism of our writers should not retaliate, and put an American dress on some of their old books. This commendable practice is gaining daily, and with the attractive prefixture of *American,* we expect to make, in a short time, naturalized citizens of their best productions. In this way we shall have as many good authors as they, and with infinitely less trouble.

It is not, however, without a grain of allowance that we give such general commendation to the labours of Dr. Currie. Where we beg leave to differ from him is in his chapter on yellow fever; but, then, in this part he is deserted by his English friends. Had he drawn upon the stores of the Italian physicians, it is probable we should be of his opinion here likewise, as we are in so many other parts of his book. Or had he an opportunity of perusing an essay which we have already noticed in this number, we should not perhaps have to controvert his opinion; for we cannot pay so bad a compliment to our own judgment, as to suppose that what is convincing to us would not be so to him. To save trouble, we shall principally refer to that essay, and we have this farther motive, with candour enough to own it, that we have nothing better to say from ourselves upon the subject. But first we submit the following passage as a specimen of the author's style, his confidence and manner of reasoning.

> "To extricate themselves from the dilemma to which they have been reduced by facts and arguments similar to the preceding, some of the leading and most influential of

the advocates of the doctrine of domestic origin of yellow fever, have had recourse to the power of imagination, and rejecting contagion as a vulgar error, have revived and adopted the more vulgar and exploded doctrine of planetary, cometic, and volcanic influence upon the constitution of the atmosphere extended round the spacious globe; and have dogmatically pronounced, without offering any but the most puerile and frivolous evidences in support of their opinion, that a noxious and impure change has taken place in the proportion of the constituent principles of the atmosphere; a doctrine which is so foreign from correct observation, and so destitute of proof, that its adoption could only be excusable in the dark age of Gothic barbarism, when the human mind, unenlightened by liberal education, and paralized by the fears of superstition and the arts of impostors, rose in intellect but a small degree above brutal instinct. In short, to ascribe the occurrence of the yellow fever in Philadelphia, after an exemption of thirty-one years, to a noxious and *invisible* change in the constitution of the atmosphere, without furnishing direct and unequivocal proofs that such change has taken place, is one of the tricks of ingenuity to impose upon and mislead unreflecting credulity, and is no more worthy of credit or respect than the Arabian Nights Entertainments, or than the conceits of the astrologers and conjurers in the ages previous to the revival of literature, when every disease as well as every natural phenomenon not obvious to the senses, was ascribed to the influence of the planets. In point of absurdity and folly it is a match for the story related by Monsieur Poqueville, of the ignorant and credulous Greeks of the present age, who, he says, believe that a decrepid spectre, to which they have given the name of Cacodæmon, always precedes the plague, dressed in a funeral shroud, and glides along the roofs of the houses, calling the names of those who are destined to be cut off from the number of the living, while dogs howl hideously responsive to solemn music and murmuring voices, which they are supposed to hear in the air."

Dr. Currie is convinced that "yellow fever is a contagious disease, and that every time it has appeared in this country, the contagion by which it is propagated has been imported from some of the West-India Islands, in the persons of the sick, or in articles of clothing, or bedding, that have been used by the sick." He also acknowledges that it is "produced by the exhalations from the corrupted bilge water confined in the ship's hold and well, particularly during the rainy season, when, from the necessity of keeping the hatches shut, fresh air is in a great measure excluded, cleanliness neglected, and the noxious exhalations permitted to accumulate." Now, if cleanliness be neglected, the air vitiated, and noxious exhalations accumulated on shore as well as in the hold of a ship, and if, besides, the temperature be sufficiently raised to give activity to these poisons, by every rule of strict reasoning the same effects must follow. It is a very gratuitous assumption that ascribes them to another cause. But the poison is diluted on shore, and thus weakened. Admitting this to be true to a certain extent, such dilution can render it innocent at a distance only from its source. The miasmata of marshes, notwithstanding the same degree of dilution, will give intermittent and remittent fevers to those who plunge in them, or live in their vicinity. In those cases where carbonic gas is largely disengaged, its peculiar effects are always felt near its source, though part of it is constantly carried off. It is needless to accumulate analogous instances: they will occur to the reader.

Dr. Currie lays great stress on the circumstance of yellow fever having appeared in this country at a time that the want of a dense population left no room for ascribing it to the filth that fills the numerous docks of our populous commercial cities. But though such causes increase the facility of its production, they are not the only ones; and those who have written on the other side of the question have proved that this disease occurs, with all its symptoms, in places that were never contaminated by the effluvia of a ship. An important point, entirely overlooked by Dr. Currie, and those who think as he does, is the temperature of the atmosphere.

Where that is tropical the disease is always endemial in marshy situations, whether it be our own country or the West-Indies. In 1805 we had yellow fever, but for the last five years we have been free from this epidemy, though some sporadic cases were met with, in some particular situations, and in conjunction with strong exciting causes. During the summers of these years the mean heat of the months of June, July, and August, observed by the same thermometer, in a cool situation, with a northern aspect, was a follows.

	June.	July.	August.
1805	69.15	76.82	76.49
1806	72.65	74.60	70.45
1807	65.83	75.33	73.35
1808	69.61		71. 9
1809	65.03	66.34	72.76

The four last years were also remarkable for frequent showers, and these always followed by north-west winds, which rapidly changed the air, whilst the moisture of the ground was evaporating. This latter circumstance must have contributed materially to the salubrity of these years.

A Treatise on a Malignant Epidemic, commonly called Spotted Fever; interspersed with Remarks on the Nature of Fever in general, &c. and an Appendix, in which is republished a number of Essays written by different Authors on this Epidemic, with the addition of original Notes: containing also a few original and selected Cases, with Clinical Remarks. By Elisha North. 12mo. pp. 249. New-York. T. & J. Swords. 1811.

THE disease which forms the subject of the present interesting volume committed great ravages in the places above mentioned for five or six years past, and especially before the medical gentlemen of those districts had discovered the most proper mode of overcoming it. Their attention was, however, soon turned to its nature and symptoms, and, by a due observance of whatever was serviceable or prejudicial in the treatment, and a free communication of their observations to each other, they performed signal services to the public, and to their profession.

"The characteristic symptoms of this disease," says Dr. North, "are pain of the head, soreness of the throat, white tongue, weak pulse, and great prostration of strength. To these are added or combined, differently in different persons, some or all of the following symptoms, viz. sickness at stomach, vomiting; sometimes, though not often, delirium, coma, chills; frequent, transitory, and evanescent flushes of febrile heat; pain and numbness of the limbs; fainting fits are frequent; spots in the skin and eruptions of various kinds and colours, some transient, others permanent. Fever is not a necessary attendant upon the disease, although some have a regular paroxysm of fever continue the usual term of ten, twelve, or twenty-four hours. These cases are not frequent. In general, although the patients have white tongue, yet they have more appetite than is common in regular fevers. Hysteric and hypocondriac symptoms are frequently an attendant or consequent of the complaint. In many the disease spins out to considerable length, as two, three, four, and even five or six weeks. I think it not contagious. It is principally confined to young people.

"Death appears to me to be occasioned, not by violence of fever, inflammation or putrefaction of the system, but by an unaccountable, sudden, and violent prostration of the energy of the brain and nervous system.

"I have prescribed for sixty-five patients with this disease during the winter; of these I have lost one. This was the second patient to whom I was called. The number of patients attended by the other physician in town I am not able at present to ascertain; sixteen have come to my knowledge. Perhaps he may have attended twenty or twenty-five; of these, four have died: one has died without a physician: three or four have been attended by physicians from out of town; as many perhaps have cured themselves; so that the whole number of the sick may amount to between ninety and one hundred. Of these, six have died; five of whom died in forty-eight hours; the other died on the fourth day of the disease. This epidemic began about the first week in December, and still continues.

" *Method of Treatment.*

" Respecting the method of treatment, the plan I have adopted, since I lost the second patient to whom I was called, has been the stimulating plan in its fullest extent, *and I can confidently say, with decided benefit in every instance.* The remedies I have been in the habit of using, are wine, brandy, opium, camphor, Peruvian bark, serpentaria, oil of peppermint, castor, elix. vitriol, rob of elder, blisters, sinapisms.

" Sweating has been much practised, and with much benefit. A decoction of the twigs of the hemlock tree, or 'Pinus Canadensis,' *Linn.* has been much used for sweating, and has accidentally become a very popular remedy; but I do not know that it is superior to other warm stimulating teas. I have, in some instances, used a gentle emetic of ipacac. but I cannot say whether with benefit or not. I have almost done using them. Gentle cathartics or clysters enter into my list of remedies, if the bowels are costive. The various methods of dosing, preparing and exhibiting the above remedies, must depend upon the practical judgment of the physician in attendance. The circumstances of age, sex, constitution of the patient, and degree of the disease of every individual patient, must vary the manner of exhibiting the different remedies."

A report on the spotted fever, as it appeared in Massachusetts, was published by Drs. Welsh, Jackson, and Warren; on this some judicious remarks are offered; they are also a summary of the treatment which succeeded best, and they conclude the volume.

" 1. When this disease first appeared at Winchester, it was conceived, partly from the symptoms of brain affection, and partly from the dissections at Medfield, in Massachusetts, that there was an inflammation of the encephalon. Hence venesection, and other depleting remedies were tried. until their baneful effects were thoroughly ascertained. See the remarks of Dr. Woodward and others. Similar experiments were made in other places with the same result. See Dr. Bestor's Essay. At length the hemlock practice was introduced by Mrs. Hurlbert, and proved successful. See Dr. Lyman's Letter to President Fitch. From that time to this nineteen-twentieths of the physicians in Connecticut, who have been conversant with this malady, have, it is believed, been in the habit of the general use, not only of stimulating and alexipharmic, but of cordial and tonic remedies. These have not, indeed, been used in all cases, *both liberally* and *indiscriminately*, as one might be led to expect from some expressions occurring in the Boston Report.

" 2. The epidemic (as appears by the Report of the committee at Boston) was treated in Worcester and Middlesex counties upon the stimulating plan, and generally with success.

" 3. In Orange county, (N. Y.) the stimulating alexipharmic practice proved successful, as stated by Dr. Arnell.

" 4. At Geneva, in Europe, after numbers had died, the remedies were found to be emetics, *cinchona*, and rarely blood-letting.

" 5. In Baltimore, tonics were found beneficial, as stated by Dr. Williamson.

" 6. I regret I am not able to state the modes of treatment which have been resorted to in the western parts of the State of Pennsylvania, and other places where this epidemic has prevailed. It is hoped the physicians in those places will, in due season, give to the public the result of their experience in this malady.

" There are those who seem disposed to resist all this weight of testimony in favour of stimulants and tonics,* and merely because the use of the remedies may have been sometimes in the hands of the unskilful misapplied. Upon similar principles we might discard the whole medical art.

" As too much evidence cannot be produced on this subject, if it is calculated to convince the sceptical of an important truth, I will again resort to the inquiry, how other malignant, or (as others with more propriety perhaps express it) ataxic fevers have been treated. Hoffman, Huxham, Tissot, Buchan, and others, and the physicians generally in Sydenham's time, used cordials, alexipharmics, or stimulants in such fevers; and they are now used in cynanche maligna, and in common typhus. Some of these physicians, at times, tried the lancet; but Dr. Wilson has shown, even from their own testimony, that its use, in typhus fever, was much too frequent. Dr. Sydenham, I am aware, as has been noticed before, opposed the physicians of his time in the use of alexipharmic remedies; but does not the result of the experiments in the spotted fever incontrovertibly prove, that Sydenham was in an error, when he applied his improvement in the treatment of small-pox to all fevers? And does it not also prove, that his opponents were the most correct in their mode of treating malignant fevers?

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* " Such, so far as I have known, have had few opportunities of forming an acquaintance with the distemper from actual observation. Even the respectable authors of the Boston Report say, that they have themselves seen but few cases of it; and I cannot but apprehend, that this circumstance, and perhaps some misinformation from Connecticut, will account for the errors which they have committed."

The authors of the Boston Report have found, upon dissection, in those who have died with the spotted fever, congestion, turgescence, effusions, slight traces of inflammation in the internal parts, lividness of the external parts, &c. &c. Hence they have concluded, that '*this disease is fever combined with internal inflammation; and the inflammation is erysipelatous.*' At other times they suppose this is phlegmonous, or approaching to it.

" To me it seems that most of the appearances after death in spotted fever, which they have so accurately described, are such as might be expected in those who die suddenly in the torpid state of fever. These are neither evidence that putrefaction, as was once believed, or, that inflammation, as is now thought, exists for any length of time previous to the death of the patient: For similar appearances have been noticed in persons who have died torpid from other causes besides fever, such as lightning, and violent passions of the mind. And no one, it is presumed, would contend that inflammation, in such cases, had preceded. Every one will recollect that Dr. Rush, when speaking of the prostrate state of fever, has produced these facts to show that putrefaction in the blood in fever, as was once taught, is improbable: for he says, " similar appearances with those which have been ascribed to putrefaction, have been produced by lightning, by violent commotions of the mind, by extreme pain, and by every thing else which produces sudden and universal disorganization in the fluids and solids of the body.'

"The gentlemen of this committee proceed to give us their own opinions respecting the proper mode of treating this epidemic; and, under the influence of the belief, that there is congestion or inflammation in the brain, recommend that blood be drawn from the jugular vein. As the directions for venesection appear to be founded more on theoretical reasons than facts, and, as their directions apply to the comatose or lethargic state, which is undoubtedly the death state of the disorder, it becomes a matter of great consequence to investigate the correctness of this doctrine. In the present state of medical knowledge, every one will allow that it must be extremely difficult to designate, by means of the symptoms, the cases in which inflammation or congestion may exist, if such cases do occur: For coma or delirium by no means always indicates this state of brain. These symptoms sometimes occur in diseases which are very opposite in their nature. And they frequently take place in diseases in which there is at least no reason to suspect inflammation; as in the cold stage of the paroxysm of intermittent fever, the death state produced by the long continued application of intense cold, in cynanche maligna, in measles. 'Coma so frequently attends the eruptive fever of measles,' says Dr. Wilson, 'that by some it is regarded as one of its diagnostic symptoms.' 'This symptom, indeed, is not to be overlooked,' Dr. Wilson continues to observe, 'in forming the diagnosis of any eruptive fever; in all of which it is more apt to supervene than in fevers properly so called.' Is not this symptom one of the cold stage symptoms of many febrile disorders? Does not the comatose state produced in those who perish by cold corroborate such an opinion? And is not restoring the heat by external warmth one of the best means of removing this symptom?

"Have not experiments been made in the spotted fever, which militate strongly against the propriety of venesection in the comatose state of this distemper? Dr. Woodward has informed us 'that he has seen patients roused by external heat, and internal stimulants, from the comatose state, and afterwards cured.' On the other hand he informs us, that 'he has seen patients brought into a low comatose state by a small bleeding, which cases entirely baffled the power of medicine, and proved fatal.' Indeed, the authors of the

Report have, notwithstanding their own theory, informed us, upon the authority of their correspondents, that in cases which have been thought desperate, fifty to a hundred drops of tincture of opium, administered every half hour, 'have almost invariably removed the lethargy.' Dr. Bestor and others have removed the coma by stimulants, and afterwards cured their patient. I have myself seen comatose patients cured by the use of external heat and other stimulants. I have, however, seen but few cases of this description; but have believed, that I have many times prevented this deplorable state by the seasonable and proper use of cordials, in conjunction with external warmth. On the other hand, almost all, as well as Dr. Woodward, who have tried bleeding, and other depleting remedies, during the first stage of the disorder, bear testimony against the practice. Venesection is pernicious in the cold stage of a paroxysm of ague and fever. It is also hurtful in cynanche maligna, and in scarlatina, especially when attended with ataxic symptoms. In malignant fevers it is generally improper. By all this I do not mean to be understood, that there may not be cases in which it may be proper to draw blood in the epidemic of which I am treating. But I believe these cases are rare, and not very well ascertained. And, notwistanding the deference due to the authors of the Boston Report, I must think that they were peculiarly unfortunate in their choice of the comatose state in the cold stage of the spotted fever for venesection. But their directions are, that blood must be drawn, by a large orifice, from the jugular vein, and cordials administered at the same time, to prevent the mischief which might otherwise ensue. But since we have satisfactory evidence that general blood-letting alone has often proved detrimental, and sometimes fatal, and also of the salutary effects produced merely by the use of stimulants, I should be cautious in admitting the conclusion derived from any other source than experiment, that stimulants would have their customary effects after the loss of so much of the vital fluid, even if it should be taken from the jugular vein. Though the patient should be relieved by the proposed mode of practice, it would be difficult to say whether the bleeding contributed, in conjunction with the cordials, to produce this change, or whether it should be attributed to the cordials alone.

"The next remedy which the committee particularly recommend is mercury. They however say, that 'in cases where life is immediately threatened, this remedy cannot be relied on to avert the danger. But in cases of less severity, or where the more urgent symptoms are removed, this is of great value.'

"I have seldom used this medicine, because, in the aggravated circumstances, which the gentlemen of this committee have first mentioned, I have believed, as they state, that it cannot be relied on to avert the danger. And, in the more favourable circumstances, I have generally found milder means sufficient to cure the disease.

"Respecting cordials we are informed, 'that there is a popular bias in their favour.' This, one would think, was strong evidence of their utility. Yet we are immediately told, 'that when liberally and indiscriminately administered in this disease, they are very injurious, by increasing the force with which the blood is thrown in the head, or other parts in which there exists inflammation or congestion.' I entirely concur with the gentlemen in the sentiment, *in the latitude it is here expressed*, respecting the use of cordials. No prudent physician, I believe, is in favour of a liberal use of cordials, administered *indiscriminately*, and to the same degree, in every case of spotted fever. In some cases a liberal use of cordials is absolutely necessary, and in others a very moderate use of the same remedies is preferable; and it requires the attention and skill of the physician properly to discriminate these cases, and to proportion the remedies to the circumstances of each particular case.

"As to the fears entertained by these gentlemen, that mischief may be effected by increasing the force with which the blood is thrown on the head, it may be observed that these are magnified. For the external hot applications, (in the utility of which every one is agreed) which have been so generally and so liberally used, must produce effects similar to those of the *hot bath*, and this is well known to determine powerfully to the head.

"Again; the Report states, 'that the very liberal use of cordials has been fully tried during the last three years in Connecticut, also during the early part of the spring in the county of Worcester. The committee have reason to believe, that a considerable proportion of the most judicious practitioners in those places are now fully convinced that this practice is highly injurious,' &c.

"If from this we are to infer, that cordials, and stimulants, and tonics, of one kind or other, are not the general remedies employed by much the greater part of the most judicious practitioners in Connecticut, such inference would be clearly incorrect. But no judicious physician would advocate their use in ordinary cases, to a degree which might perhaps be termed by some *very liberal,* much less in any case to an excessive or extravagant degree. The quantities, however, as has already been observed, have varied exceedingly, according to the circumstances of the patient; and yet the above remedies are almost universally relied upon for the cure of spotted fever. And if the gentlemen at Boston have received information different from this, I have no hesitation in pronouncing it incorrect. That the use of cordials may not have been sometimes abused in Connecticut, either through inadvertance or want of skill, it might be presumptuous to deny; and yet that their judicious administration has been attended with the most cheering success, the voice of the medical faculty, and that of the public in general, will, I think, bear united and ample attestation."

Some of the physicians who first saw this fever supposed it to be a new disease, but to us it appears to be simply typhus, with some variety in the symptoms, a thing not unusual to this disorder in different seasons and constitutions.

Observations on Combustion and Acidification; with a new Theory of those Processes, founded on the Conjunction of the Phlogistic and Antiphlogistic Doctrines. By John Redman Coxe, *M. D. Professor of Chemistry in the University of Pennsylvania.* 12mo. pp. 50. *Philadelphia. Published by the Author.* 1811.

PROFESSOR COXE acquaints us that these observations formed the substance of a lecture delivered to his class, in which he endeavoured to reconcile the contending systems of the phlogistians and the antiplogistians. The Professor reasons with ingenuity, and makes several plausible conjectures, but adds no new observations or experiments to throw light upon this controversy.

INTELLIGENCE.

DOMESTIC.

Medical School of New-York.

THIS institution, of which some account has been given in our preceding numbers, formed under the fostering care of the Regents of the University, and endowed by the liberality of the Legislature,* was advancing with a rapidity which exceeded the most sanguine expectations of its warmest friends. Although opposed by a rival school, it counted no less than seventy-three pupils attending its courses of instruction the third winter of its existence; a greater number than had ever attended any similar seminary in this city. But the fourth winter presented a lamentable reverse, the number of students being reduced to less than one half. Upon this being reported to the Regents, they appointed a Committee† to inquire into the subject, who presented the following report, which was unanimously adopted.

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* November, 1808, the Legislature gave the College $20,000, and at their late session they made it a further grant of $500 per annum. At the same time the Regents placed under the direction of the College the Botanic Garden purchased by the State of Dr. David Hosack, at an expense of upwards of $74,000.

† The committee consisted of Chief Justice Kent, Judge Spencer, and Judge Smith.

“ *At a meeting of the Regents of the University, held, pursuant to adjournment, in the Senate Chamber, on the first of April,* 1811:

“ The committee to whom was referred several papers relative to the state of the College of Physicians and Surgeons in the city of New-York, report, that unfortunate misunderstandings have taken place between several professors of that institution, which have already materially impeded its operations, and unless something effectual be done by the Regents, it will become degraded in the estimation of the public, and its usefulness will be inevitably destroyed.

“ The committee have forborne to trace and bring to light the conduct of individuals, because in their opinion it would be both useless and inviduous.

“ Propositions have been made to the committee to remodel the institution, with a view of rendering its operation more simple, and of introducing into it several of the professors of the medical school in Columbia college, and other eminent and distinguished individuals. This proposition has been viewed by the committee in the most favourable light, as it may extinguish the feuds existing among the present professors of the college of physicians and surgeons, and as it will, in all probability, be the means of uniting the two schools.

“ The latter appears an object of the first importance, in as much as it will assemble, in one institution, a splendid collection of medical and surgical talents, and as it cannot fail to merit and receive the patronage and encouragement of the Legislature.

“ It is unnecessary for the committee to attempt to display the important advantages to the State which a well organized medical school in the city of New-York must afford; its hospital, and the subjects furnished by the state prison, without the violation of law, present a field for the acquisition of medical and surgical knowledge unrivalled in the United States, and it is only requisite to establish an institution, under the fostering care of the Legislature, in which shall be united the best talents, to secure these advantages to the State.

" Under these impressions the committee beg leave to report an alteration of the charter of the college of physicians and surgeons, and to propose a new list of officers and professors, which alteration is as follows, to wit:

" *By the Regents of the University of the State of New-York.*

" Whereas we have reserved to ourselves the right to alter and modify our ordinances for establishing a college of physicians and surgeons in New-York; therefore,

" *Be it ordained,* That all the corporate rights, privileges, powers, and immunities granted by us to the said college of physicians and surgeons, to the trustees or members thereof, and to the senatus academicus and censors thereof, shall hereafter be exclusively vested in, and exercised by the trustees of the said college of physicians and surgeons, to be appointed from time to time by us or our successors. And the said trustees shall do and perform all matters and things which the said college of physicians and surgeons, the trustees or members thereof, and the senatus academicus and censors thereof, are authorized and required to do and perform.

" *And be it further ordained,* That the president, vice-president, professors, and treasurer of the said college for the time being, and such other persons as we, or our successors, may hereafter appoint, shall be the trustees of the said college, provided the whole number of the said trustees shall not, at any time, be more than twenty-five.

" *And be it further ordained,* That the president, or vice-president; or any three of the trustees shall have power, at any time, to call a meeting of the said trustees, by giving at least three days previous notice of the time and place of holding said meeting. And five of the said trustees (of which the president, or, in his absence, the vice-president, shall be one) shall be a quorum for the transaction of business.

" *And be it further ordained,* That the trustees and members of the said college, who are not constituted trustees by this supplementary charter, shall be hereafter fellows or members thereof, and that the trustees shall have power to elect fellows or members of the said college, who shall at all times have the privilege of attending all the public lectures and other courses of instruction, delivered by the professors in the said college; and who shall also have the privilege of visiting and inspecting the anatomical museum, the botanic garden, the cabinets of mineralogy and natural history, and the library of the said college, under such regulations as the trustees shall prescribe for that purpose.

" *And be it further ordained,* That reserving to ourselves and our successors, regents of the university, the power of making such other grants or ordinances as may be necessary or useful for the said college, we finally order, that this ordinance shall form part of the charter of the said college of physicians and surgeons in the city of New-York. And we do hereby revoke and annul such parts of our previous grants and ordinances as are contrary to, or inconsistent with the present ordinance.

" *A true extract from the minutes,*

" FR. **BLOODGOOD,** Secretary.

" *At a meeting of the Regents of the University, held, pursuant to adjournment, in the Senate Chamber, on the* 1st *of April.* 1811:

" RESOLVED, That the following appointments be and hereby are made in the College of Pysicians and Surgeons in the city of New-York, to wit:

" Samuel Bard, M. D. President.

" Benjamin De Witt, M. D. Vice-President.

" Wright Post and John Augustine Smith, Joint Professors of Anatomy, Surgery, and Physiology.

" David Hosack, M. D. Professor of the Theory and Practice of Physic and Clinical Medicine.

" William James Mac Neven, M. D. Professor of Chemistry.

" John R. B. Rodgers, M. D. Professor of Obstetrics, and the Diseases of Women and Children.

" Edward Miller, M. D. Professor of Therapeutrics, and Clinical Medicine.

" William Hamersley, M. D. Professor of the Institutes of Medicine.

" Samuel L. Mitchill, M. D. Professor of Natural History.

" James S. Stringham, M. D. Professor of Medical Jurisprudence.

" Archibald Bruce, M. D. Professor of Mineralogy and Pharmacy.*

" John D. Jacques, M. D. Treasurer.

" John W. Francis, Register.

" And that all former appointments made in the said College of Physicians and Surgeons be, and the same are hereby revoked.

" *A true extract from the Minutes,*

" Fr. BLOODGOOD, Secretary."

By the foregoing proceedings of the Regents the College has been entirely re-modelled, and its operations simplified, and, consequently, better adapted to insure the great object of the institution—the advancement of medical science. The good effects of the change are indeed already apparent, as there has been, since it was effected, a commencement, when the following eight* young gentlemen received the degree of Doctor in Medicine, after having publicly defended their several inaugural dissertations.

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* At this time a paragraph appeared in the papers stating that Col. Gibbs was made Professor of Mineralogy, and it was ludicrous enough to see with what haste that gentleman advertised his non-acceptance of what was never offered to him.

Theoderic Romeyne Beck, A. M. of Schenectady, " On Insanity."

William E. Burrell, A. B. of New-York, " On Bubonocele."

Gerardus A. Cooper, of New-York, " On Worms in the Intestines."

Caspar Wistar Eddy, of New-York, " On the Analogy between the Animal and Vegetable Structure."

John W. Francis, A. B. of New-York, " On Mercury."

Henry Ravenel, of South-Carolina, " On Trismus Nascentium."

Thomas Edward Steel, A. M. of New-Jersey, " On Digitalis Purpurea."

Samuel Armstrong Walsh, A. B. of New-York, " On the Origin and Nature of Yellow Fever."

The College having thus gone completely into operation, no further material impediment to its progress will, it is to be hoped, occur. All the gentlemen, it is true, who were appointed have not thought proper to accept, but a sufficient number have undertaken to discharge the duties assigned them, to give complete courses of medical instruction. For further information upon this subject, we refer the reader to the circular of the College at the end of this number.

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* This was nearly double the number of medical degrees ever before conferred at one time in New-York. The ceremony was performed by Dr. Samuel Bard, the venerable President of the College, in the presence of the Governor of the State, the Mayor of the City, several other Regents, Trustees of Columbia College, &c. &c.

In all seminaries of learning it is customary to crown with academic honours those whose acquirements entitle them to that distinction. As these should, in every case, be conferred on merit, and merit only, the Regents have ordained, that any candidate may offer himself for examination to the Professors of this College, though he should not have previously studied in any regular university: the principal object with them being to ascertain his learning, and not where he acquired it. But then the examination must be minute and strict, and the Professors are bound to report annually before the first of March the names of those whom they deem worthy to receive degrees. In every instance where the report is favourable, the Regents grant the degrees. They are conferred at a public commencement, held the first Tuesday in May. For a public exercise each graduate must write a dissertation on some subject connected with medicine, which, if approved of by three professors, he maintains at a public examination; but he is at liberty to publish it or not, as he may choose.

Spirit of Turpentine.

We are sorry to state, that the spirit of turpentine, which has been of late extolled as a cure for the tape worm, has failed in a case in which we have known it used. The subject of the disease was a woman, and she swallowed, at one dose, fully two ounces of the rectified oil of turpentine. The first sensation which it produced was that of warmth in the stomach, and its ultimate effect was that of a violent cathartic. Some temporary relief from her complaint was obtained, but in a short time it returned with unabated violence.

Although the spirit of turpentine failed in the above instance, we think it worthy of further trial.

Bill of Mortality for Portsmouth, New-Hampshire, for A. D. 1810. *By Lyman Spalding, M. D.*

Complaint.	*Age.*	*No.*
Apoplexy	78‘ years.	1
Atrophy	1, w 76‘ y 3 d 2, 5, 68‘ y 2, m 2, 47‘ y 10. d 4. m 56, 2. y	13
Chlorosis	13, years	1
Cholera of infants	4. m 2. 2, y 1, m 1. 1. 1, year	7
Consumption	33. 35‘ 36. 23, 63‘ 35 64‘ 58, 20, 21, 42 37· 25‘ 52 69 40· 15, 41‘ 38‘ 41‘ 23. 28, 17, 25‘ 44‘ 28· 34. y.	27
Convulsions	5. m 2, y 6. m 4. years	4
Croup	2, years	1
Dropsy	40‘ 60‘ 2. years	3
Dropsy in the brain	2. 13, y 5. m 2. 5, 4, 2, y 3, months	8
Erythema	3. m 1, week	2
Fever, inflammatory	5. 4, years	2
Fever, petechial	9, years	1
Fever, puerperal	18‘ 44‘ years	2
Fever, pulmonic	52‘ 75‘ 78‘ 5, y 4, months	5
Gout	74· years	1
Hemorrhage	69· 52‘ years	2
Hooping cough	1, 3, years	2
Inflammation	46· years	1
Mortification	53‘ 67‘ years	2
Old age	89· 90‘ 77‘ 89‘ 90· 79‘ 78· years	7
Palsy	58‘ 74· 87‘ 60‘ years	4
Phrenitis	50· years	1
Quinsy	7. m 1. 1, year	3
Scirrhus liver	25· years	1
Casualities.		
Burnt	75‘ 4, 1, year	3
Drowned	2. 7. years	2
Overlaid	2. months	1
Run over by a waggon	45· 5. years	2
Suicide	19. 25‘ years	2
Births.		
Males	130	
Females	122	
Still born	6	
Marriages	64	
	Total	111

Portsmouth, the capital of the State of New-Hampshire, situated

43° 5′ north latitude, and 6° 26′ east longitude from Washington, contains 6934 inhabitants.

The month of January exhibited the coldest, as well as the most remarkable change of weather ever recorded in New-Hampshire. At 12 o'clock at noon of the 18th the thermometer was at 42°, and at 12 the 19th it had fallen 12° below zero. It fluctuated between 7° and 14° below zero from the 19th to the 22d. We do not find that either this sudden change, or extreme cold weather had any material effect on the health of our citizens.

Note. This bill is so constructed as to show the complaint, age, sex, &c. and whether married or unmarried. When a period follows the age, it denotes the male sex, a comma the female sex; when in their usual place at the bottom of the line, unmarried—at the top of the line, married.

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FOREIGN.

[*The following paper is extracted from the Edinburgh Review on account of its value, and because the original pieces were inserted in the Philosophical Transactions of the Royal Society, a work which few of our readers have an opportunity of seeing.*]

A Letter on the differences in the Structure of Calculi, which arise from their being formed in different Parts of the Urinary Passages; and on the Effects that are produced on them by the internal Use of Solvent Medicines, from Mr. William Brande to Everard Home, Esq. F. R. S. (From the Phil. Trans. for 1808, *Part* II.*)*

Some Observations on Mr. Brande's Paper on Calculi. By Everard Home, Esq. F. R. S. (From the same volume.)

An Account of a Calculus from the Human Bladder, of un-

common Magnitude. By Sir James Earle, F. R. S. (From Phil. Trans. for 1809, Part II.)

Observations on the Effects of Magnesia in preventing an increased formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr. William T. Brande, F. R. S. to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society. (From the Phil. Trans. for 1810, Part I.)

On Cystic Oxide, a new Species of Urinary Calculus. By William Hyde Wollaston, M. D. Sec. R. S. (From the same work, Part II.)

These curious and valuable papers relate to one of the most excruciating torments with which the Divine Providence has, for wise purposes, decreed that the lot of man should be mingled, during his existence in this world. Indeed, were it not that custom teaches us to consider mental afflictions as, in the general, more severe than any bodily pains, (and, no doubt, to some natures, they are, beyond all comparison, more unbearable) we should be disposed to rank the dreadful complaint which we are about to treat of, as the worst of human maladies. To investigate the nature of the stone, for the purpose of discovering solvents which might remove it, has accordingly been long considered as one of the noblest problems in practical chemistry, and among the best services which that science could render to the healing art. It may be remembered, that to this investigation we owe the brilliant discoveries of Dr. Black, which, by disclosing the nature of fixed air and of the alkaline earths, paved the way for all the modern improvements in chemical knowledge. Those great acquirements in speculative science were the compensations which he obtained for his disappointment in a search far more important and interesting to humanity,—as the navigators of the fifteenth and sixteenth centuries discovered the new world, and explored its strange recesses, while occupied in the pursuit of an earthly paradise, or a fountain of health and youth. Even the legislature of this country took some part in those inquiries, and gave rewards to the discoveries of solvents. The most noted instance is that of Stevenson; and here the money was granted upon the faith of a cure *said* to be performed, the body not having been examined after death.* We need scarcely add, that the medicine thus patronized, and, as it were privileged, has long since been completely exploded; and though, in its failure, the Parliament resembled the celebrated philosopher whose name we have mentioned, we do not remember that its investigations were, like his, rewarded by any other discoveries.

The hopelessness of the search for *solvents* has lately turned the attention of the chemists and physiologists to another problem; and they have been occupied in ascertaining the process of calculous formations, with the view of administering such *preventive* medicines as may indispose the system to produce those concretions, or check their growth, without altering the tone of the constitution. The papers now before us throw considerable light on this important subject; and contain also a good deal of information, tending to elucidate the fallacies of the older doctrines respecting solvents. Indeed, did we not know the danger of trusting to medical reports of cures, and did we not recollect how regularly a series of cases, all quite apt and most nicely fitting the doctrines, forms a part of every modern treatise on medical subjects, we should be tempted to consider a great part of the question solved with regard to the stone, and should confidently look to the extirpation of that worst of maladies. Even moderated by such unpleasant recollection, as we own our hopes to be, we cannot look to the eminent names prefixed to these tracts without great respect; and we feel at least so much inclined to listen, that we shall make no farther apology for laying before our readers an account of what the papers have added to the stock of our knowledge on this subject.

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* The following history of this affair is given by Mr. Cline, in his lectures. Upon application being made to the British Parliament by Mrs. Stevenson, for a reward for her discovery, they directed that the remedy should be fairly tried. For this purpose a patient was sounded, and it being clearly ascertained that he had a stone, he was put under the care of Mrs. S. From the time he began to take her medicines his symptoms became more favourable, and he was at length returned to the surgeons who had formerly sounded him, as cured. They proceeded to sound him again, but could now find no stone. They, however, would not say that there was not a stone in the bladder, but stated that they could not now feel one there. This was thought sufficient, and the reward was granted. Upon the man's death his body was opened, when the stone was discovered in a cyst, (a very rare occurrence indeed) in which it was so completely enveloped as to give the patient no further inconvenience, nor could it be touched by the sound.

In order to convey some idea of the greatest degree in which this disease has ever yet, we believe, been known to afflict its wretched victims, we shall begin with noticing the case narrated in Sir J. Earle's paper.

Sir Walter Ogilvie, a Scotch Baronet, received, at the age of twenty-three, a severe blow on the back, from the boom of a vessel; and the pelvis and lower extremities were in consequence paralyzed. He remained in bed for fourteen months, in an horizontal position; and, during the first two months, his urine was of necessity drawn off by a catheter. After this he had somewhat recovered the use of his limbs, and could walk or ride, though with difficulty: his general state of health, too, was weak and precarious. About twenty years after the accident he was afflicted with symptoms of stone; and, upon examination, a calculus of considerable size was found to have been formed in the bladder. An operation was now recommended, but, unfortunately, delayed from time to time, although the patient's health constantly declined, and the irritations and pains in the bladder constantly increased. In this melancholy state he continued for eight years longer, when he became unable to make water in an upright position: he was obliged to place himself in an inverted posture, that the upper part of the bladder might become the lower; and as, even by this means, very small quantities of urine only could be voided, the irritation of endeavouring to make it became almost perpetual, and he was obliged to perform the above mentioned operation every ten minutes, with scarcely any relief. The principal remedies which he tried were *aqua calcis* and *uva ursi*. He used opium also to alleviate the pain; and this, adding to a naturally costive habit, rendered aperient medicines frequently necessary, which, again, must have increased the irritations of the disease. At the age of fifty-three, thirty years after the accident, the spasms became so constant and so violent, that he resolved to have the operation performed; and the paralysis of the lower parts being deemed no obstacle to it, he was conveyed by sea to London, where Sir James Earle and Mr. Cline consulted upon his truly lamentable case, about the end of July, 1808.

Upon examining the lower part of the belly, a tumour was perceived, so large, that Sir James Earle doubted its being altogether caused by the stone; but, on sounding, the instrument would not enter the bladder; and further examination showed that the whole cavity was nearly filled with calculus. It was clear, therefore, that this could only be extracted, if it should prove soft enough to admit of being broken, and thus brought away peacemeal; and it was deemed impossible to attempt the operation above the *os pubis*. The ordinary method was therefore resorted to: and, after some days consideration, Sir Walter Ogilvie, well aware of the difficulties of the case, and of its great uncertainty, yet justly weary of the long life of torment which he had led, was resolved to have the operation performed, for the chance which it afforded of bringing a relief, only otherwise to be attained by death. It was accordingly performed by Mr. Cline, on the 11th of August.

He was placed in the usual position, and the ordinary ligatures were applied; but the paralysis of the lower limbs rendered it unnecessary to confine them, and they were left at liberty. It was found impossible to pass the staff, or any kind of forceps, into the bladder; but, on pressing hard with the finger, part of the stone felt soft, and gave way. This making room for the forceps and scoop, some parts of the concretion were brought away; and, in this manner, about a large teacupful was extracted. The great mass, however, continued hard and immoveable: it could neither be broken nor shaken from its position; and, after trying every instrument, of all powers, the operator was compelled to desist, leaving it (with the exception of the soft matter brought out) in its former state. The patient bore this long process with great fortitude; happily, the paralysis of his lower regions may be supposed to have somewhat diminished the sensibility of the parts. He was, however, completely exhausted, from weakness and fatigue.

Although no hæmorrhage, nor any fatal symptom, appeared to result from the operation, it had afforded no sort of relief; and the spasms continued as before. They soon increased in frequency and effect, as he became exhausted, and unable to stand them. He seems to have had a gleam of hope on the morrow of the operation; for he frequently inquired when he might be well enough to have the remainder extracted. But he gradually sunk under the repeated and violent irritations of the spasms. On the eighth day his pulse became smaller and quicker, and he could scarcely be prevailed on to take any sustenance: on the ninth he was more impatient, feverish, and restless; and, on the tenth day "he desired not to be teazed to take any thing more; when, covering himself completely with the bed-clothes, he quietly resigned a most singularly miserable existence."

On dissection, the bladder was found to be *quite filled* with a huge calculus, composed of a triple phosphate, which we shall afterwards have occasion to speak of; and of such an unusual proportion of animal matter, that it speedily showed a strong tendency to putridity, and even engendered maggots. It weighed forty-four ounces, or three pounds four ounces, apothecary's weight. Its form was that of an irregular illipsoid; the larger circumference being sixteen inches, and the smaller fourteen. It had filled the bladder, and taken its shape, but apparently without distending it. The ureters had become enlarged, so as to contain the urine, and act as small bladders; while the large bladder only permitted that fluid to pass off by means of small furrows which it made in trickling along the surface of the stone, between the stone and the bladder. There appears no reason to doubt, that, in a short time, even this painful and insufficient operation would have become impracticable, by the further enlargement of the stone stopping up the mouths of the ureters; in which event, death must have ensued from suppression.

Such, in its very worst state, no doubt, is the dreadful malady which forms the subject of the chemical papers now before us. To these we must now direct our attention; and we can promise the reader sufficient reward for his trouble, if he shall be pleased to follow us through the analysis of their contents.

Mr. Brande's valuable paper, which stands first in the prefixed list, is founded on an examination of the collection of calculi contained in the Hunterian Museum. Besides the known richness of that collection, most of the specimens have histories of the cases annexed to them, which are obviously of the greatest use in the investigation of the subject. Mr. Brande arranges calculi into four classes—first, Those which are formed in the kidneys, and voided without undergoing any change in the passage; second, Those which are retained in the kidneys; third, Those which are found in the bladder; and, fourth, Those which are found in the urethra. On each of these species, our author has made a number of experiments; and the results exhibit a sufficiently distinct account of their chemical analysis. The first class, or calculi of the kidneys, consist almost always of uric acid (the Lithic acid of Scheele) and animal matter; —the animal matter is found in various proportions, from one-eighteenth to one-third of the whole compound. Sometimes the acid and the matter are not formed into a solid calculus, but pass off mixed with phosphates. The author, however, is disposed to think that the phosphates are not elaborated in the kidney, but mingle with the calculus matter in its passage. In some few instances the calculus contains oxalate of lime; but this is very rare. The size of these calculi varies, and one has been described by Dr. Heberden so large as to weigh twenty-eight grains; but it may have been detained on its passage, and so augmented in bulk. The largest which Mr. Brande examined only weighed seventeen grains. All the calculi of this class are certainly soluble in pure potash; but it is needless to add, that the caustic qualities of this substance render its application in the living subject impossible.

The second class is formed by the detention of a calculus in the kidney, while either the secretion of uric acid enlarges its bulk, sometimes so as to form a complete cast of the pelvis of the kidney; or the stream of urine to which it is exposed deposits the phosphates upon the original nucleus of uric acid and animal matter. In the one case, we have an enlarged calculus of the first class—in the other, a calculus of the third class, only detained in the kidney.

When the uric calculus passes into the bladder, and is there detained, or when a calculus of oxalate of lime comes down from the kidney—or when portions of sand or animal mucus are deposited in the bladder—or when any extraneous substance has found its way thither and is detained—in all these cases, that is, on all these substances as nuceli, there are formed calculi (commonly termed stones in the bladder) with greater or less rapidity, according to the tendencies of the constitution. They may be ranged into three subdivisions—those composed chiefly of the uric acid—those chiefly composed of phosphates (the ammoniaco-magnesian phosphate, or phosphate of lime, or a mixture of the two); and those containing oxalate of lime, which are commonly termed mulberry calculi. Messieurs Fourcroy and Vauquelin, and Dr. Pearson, having found the greater number of calculi to be composed of uric acid, (that is to say, belonging to the first of these subdivisions), our author was surprized to find, that in the Hunterian collection this proportion did not hold: for, of a hundred and fifty which he examined, only sixteen were composed of uric acid wholly (beside the animal matter),—forty-five of that acid with a small portion of phosphates,—and five of the acid with phosphates and nuclei of oxalate of lime; while sixty-six contained phosphates with a small admixture of uric acid,—twelve the phosphates entirely,—and six chiefly oxalate of lime. Observing that a greater loss was sustained in obtaining uric acid from calculi in the bladder, than from calculi in the kidneys, Mr. Brande was led to conclude that the former contained urea. With a view to ascertain this point, he made some experiments; and found that an uric calculus, weighing 60 grains, contained 5.2 of urea and muriate of ammonia, 6 of ammoniaco-magnesian phosphate, and 48.8 of uric acid—but no urate of ammonia could be detected: from whence, and from other experiments, our author infers, that this substance, when supposed to exist in calculi, owes its origin to the developement of ammonia, and its recomposition with uric acid in the course of the processes of decomposition. The mulberry calculi (which are rare) contain, in general, a considerable admixture of phosphates and uric acid with the oxalate of lime. The very large calculi often contain an agglomeration of different species of calculi. Mr. Brande examined one, of twenty-three ounces and twenty-six grains, which had a nucleus of uric acid, surrounded with a considerable quantity of very pure ammoniaco-magnesian phosphate: the mass of the stone consisted of mulberry calculus. Another, weighing 15½ ounces, had a nucleus of uric calculus, round which was a mass of ammoniaco-magnesian phosphate, intersected with lamina of uric acid. It is rare to find calculi composed of four distinct substances. Our author saw one which had four separate layers, or strata, of uric acid, oxalate of lime, ammoniaco-magnesian phosphate, or phosphate of lime. He examined four calculi, the nuclei of which were extraneous substances, viz. a garden pea, a needle, a hazle nut, and part of a bougie. Besides the animal matter, the first calculi contained phosphates only; the second, phosphates and oxalate; and the other two were also destitute of uric acid.

The fourth class of calculi, those found in the urethra, offered no peculiarities to the observer. Indeed, the classification of Mr. Brande does not refer so much to any distinct properties of the substances arranged, as to their position and accidents—their *habitat,* as the botanists express it; for their properties seem to depend merely upon their exposure to the urine in the different stages of its elaboration; nor is there any manifest variety of circumstances affecting their formation, except in so far as they are found in streams of that fluid in its nascent or in its perfect state. The calculi in the urethra, accordingly, are only small calculi, which, passing from the bladder, have been detained and lodged in the membranous part of that canal, and there have been exposed to the same process of slow accretion, from the passage of urine, which they would otherwise have undergone, from lying in the bladder exposed to the action of the same fluid. One of the calculi of this description which Mr. Brande examined was of a singular appearance. It was a perfect sphere, of about half an inch in diameter, coated with small regular crystals of the ammoniaco-magnesian phosphate in its purest state.

Our author closed his experiments with examining the calculi of different animals. In the horse, sheep, rhinoceros, and rabbit, the calculi consisted of the phosphates in part, with animal matter and carbonate of lime. In the dog no carbonate of lime was found: and, in the hog and ox, no phosphates,—the calculus being wholly composed of carbonate of lime and animal matter. No uric acid nor oxalate seems to have been detected in any of the specimens. Some of them were of a large size: a calculus from an old dog weighed sixteen ounces.

From this induction, it is very evident that one conclusion may safely be drawn—that, in most instances, the stone is connected with the formation of uric acid. Sometimes that acid forms calculi in the kidneys, which, after creating sufficient torment in that region, pass into the bladder, and become the nuclei of still larger, more painful, and more fatal accretions of other substances; while, in many instances, the same acid continues to augment the mass, either alone or combined with other substances. To prevent its formation, then, becomes a most important object; and both Mr. Brande and Mr. Home propose, for this purpose, the exhibition of some alkaline earth, which may neutralize and carry it off. But, before adverting to this preventive remedy, we may notice the additional proofs adduced, in the papers now under review, to evince the hopelessness of any attempts to solve the stones already formed, at least in the present state of chemical science.

In arguing this point, Mr. Brande (or, as Mr. Home always terms him, Mr. W. Brande) proceeds more by principle;—his patron, Mr. Home, comes forward to his protection with cases. Mr. Brande clearly shows, that an alkali administered to a calculous patient stands no chance of reaching the uric concretion in a caustic state; for the urine contains both phosphoric and carbonic acid uncombined. But experiment clearly shows, that neither carbonates nor subcarbonates exert any sensible action on uric acid: in other words, (as we apprehend) the affinity of the uric acid for alkalies is weaker than the affinity of carbonic acid for the same bodies: therefore, alkaline liquors cannot act as solvents of the uric calculi. On the other hand, if acids be administered with a view of attacking the phosphates, the formation of the uric acid is greatly favoured. This, Mr. Brande's experiments have also taught him. And as for the injection of solvents through the urethra into the bladder, he justly observes, that although we were not ignorant (as we are) of the kind of calculus in each case, the frequent introduction of instruments into the bladder, and the long continuance of the operation, would form insuperable obstacles to this mode of treatment, which, though recommended by great names, has always been speedily relinquished when tried.

Now come Mr. Home's cases,—which plainly demonstrate, that where the use of alkaline medicines was supposed to have relieved the patient by dissolving the calculus, on examination no such effect was found to be produced. In two cases, where the violence of the complaint appeared to have subsided, the dissolution was taken for granted, and ascribed to alkaline medicines; but, on dissection, the calculi were found in great size, only imbedded in cysts, from the enlargement of the prostrate gland, which often takes place late in life. In some instances, again, the increase of calculous concretions was found to proceed rapidly, while the patients were going on with courses of alkaline medicines. One person took these remedies for four or five years, and, at his death, the bladder was found nearly filled with light, spongy calculi of different sizes, no less than 350 in number. Another, who had taken soda, both mild and caustic, for some months, and then submitted to the operation on the symptoms increasing, was found to have a calculus, which was surrounded with a coat of triple phosphate, one-tenth of an inch thick, the rest being a mixture of uric acid and phosphates; from whence it is fair to infer, that the alkali increased the formation of triple phosphate, though it checked the production of uric acid. In a third case, of a very virulent nature, the exhibition of alkalies did not even prevent the formation of uric concretions. Mr. Brande's experiments, however, in 1808, having rendered it highly probable, that, in the great majority of cases, alkaline medicines would operate powerfully in checking the growth of uric calculi, by neutralizing the uric acid before it could form a concretion, this inquiry has been lately resumed by that industrious and skilful observer, with the aid and advice of Mr. Home; and the result of their joint labours is given in the fourth of the papers examined at the head of this article.

Upon consultation with Mr. Hatchett, they were led to fix upon magnesia, in preference to the other alkaline substances; and the event appears to have justified this very natural and judicious choice. Some preliminary trials showed, that the mild, safe, and easily prepared earth in question diminished the quantity of uric acid in urine, which contained a larger proportion of it than any of the alkalies, however copiously administered. But this point is of such primary importance, that we must notice the four cases in which a further examination of the matter was prosecuted.

The first was that of a gentleman sixty years old, who, apparently from the habit of drinking acid liquor, had acquired an uncommon tendency to secrete uric acid, and had his urine constantly mixed with that substance, in the form of red sand or crystals. He took, first, the subcarbonate of soda, and then of potash, without any amendment from the former, and with but a slight relief from the latter. His urine being previously examined, he was ordered to take fifteen grains of magnesia three times a day, and in a week the uric concretions diminished sensibly—in three weeks they were only observed occasionally. The same medicine has been continued for eight months, and no calculi have been voided; nor has there been any material deposit in the urine. He has also been cured of heartburn, and other stomach complaints. Another gentleman of about forty years of age, who suffered greatly from a similar complaint, and tried the alkalies in vain, has been wholly cured by a six weeks' course of magnesia, in the quantity "of twenty grains every night and morning," (we wish Mr. Brande would say more distinctly whether he means *twenty* grains a day, or *forty*) without any change of regimen whatever. The third case is that of a gentleman forty-years old, who has, for about a year, succeeded in driving away repeated attacks of the uric acid, by taking magnesia for a few weeks at a time, when the fit comes on. The fourth case is that of a more confirmed calculous tendency, which has nevertheless yielded, in a great degree, to the free use of magnesia; with this addition, that since the patient began it, he has been free from gout, contrary to his former habits.

The different qualities of magnesia and the alkalies thus observed, led our author to make some experiments on their respective effects upon *healthy* urine. Subcarbonate of potash and soda occasioned a copious precipitation of the phosphates in the urine, during the first two hours after these salts were taken into the stomach; and, after that, no further effect was produced. The same alkalies, saturated with carbonic acid, threw down the phosphates less copiously and rapidly. Magnesia, administered in the same circumstances, produced a much smaller and slower precipitation. Instead of this effect reaching its maximum in a quarter of an hour, as it did in the case of the alkalies, it did not reach that point in less than six hours; and on this circumstance our author concludes, that its value in calculous disorders chiefly depends. Lime, either in the caustic or mild form, produces a very slight effect; and its nauseous taste, as well as the difficulty of administering it in sufficient quantities, manifestly preclude its use in this disease. In one case, carbonic acid exhibited in aërated water, was found to keep the phosphates (which the urine was greatly charged with) in a state of solution; and, when left off, those salts were again voided in their solid shape.

Thus far, then, we think, it may safely be hoped, at least with as little risk of disappointment as usually attends our speculations in medical science, that some light has been thrown on the method of treating a disease, of all others, which prey upon the body, the most inimical to the happiness and comfort of man. Where the object is so valuable, it is natural for us to feel uncommonly anxious and distrustful, as well as more than usually eager in our wishes for its accomplishment. Instead, therefore, of being satisfied with what Mr. Brande and Mr. Home have already effected, we would expect them to persevere in their experimental inquiries—multiplying the number of their observations on real cases—and, as it were, leaving no stone unturned to attain the complete mastery of this great problem. Much will be done for medicine, if they should go no further than perfecting the preventive application of magnesia to the uric calculus. A great number of calculous cases, of the worst description, will be prevented,—those, to wit, in which the stone in the bladder is formed on a uric nucleus. Many others of the same class will be greatly relieved. Those in which composite calculi consist partially of uric acid, and all that class of complaints with which the region of the kidneys is afflicted, by the uric concretions formed there, will be entirely removed. Should such a step in the healing art be made, we need scarcely despair of living to see the phosphates themselves attacked by it, and yielding to some equally simple and safe remedy.

While we are, however, indulging in these visions, and anticipating the final eradication of all calculous disorders, we unluckily cast our eye on the last of the papers which are now before us, and find, that Dr. Woollaston has been discovering a new species of urinary calculus, at the very moment that his learned brethren were occupied in extirpating the already sufficiently numerous specimens of the old catalogue. This new species is extremely rare, our author having only met with it twice. It resembles the triple phosphate more than any other kind; but is more compact, and consists, not of luminæ, but of a confused mass of crystals, having a yellowish lustre and semitransparency; but it is formed of a peculiar substance. Dr. Woollaston made as many experiments on this substance as the limited quantity of it in his possession would allow. When burnt, it gives a smell quite peculiar to itself. When distilled, it gives a fetid carbonic of ammonia, and an animal oil, leaving a spongy coal. It is readily dissolved by all the pure alkalies and by lime-water—it is even solved by the carbonates of potash and soda. The acids dissolve it also, except the citric, tartaric, and acetic. Neither water, alcohol, nor saturated carbonate of ammonia dissolve this singular substance. From its disposition to unite with both acids and alkalies, Dr. Woollaston suspects it to be an oxide; which is confirmed by its forming carbonic acid in distillation; that is to say, if we take it for granted, as he seems to do, that this acid does not exist ready formed in the mass. If it does contain oxygen, our author admits, it must be in a quantity insufficient to give it acid properties; for it produces no effect on the colour of litmus paper. On the supposition of its being an oxide, and to distinguish it from other bodies of that class, Dr. Woollaston proposes to name it the *cystic oxide,* on account of its having hitherto been only found in the bladder.

This excellent chemist concludes his paper with some curious observations on the connection between the production of uric acid in birds, and their food. The white matter contained in their urine, and voided along with their dung, was found by M. Vauquelin to consist principally of that acid; and our author examined with some care in what manner its quantity was affected by the diet of those animals. In the dung of a goose fed on grass it only formed $\frac{1}{300}$ part;—in that of a pheasant fed on barley it amounted to $\frac{1}{14}$;—in that of a hen which fed on insects as well as vegetable food, in a barn-yard, it was much more abundant, and mixed with lime. The solid part of the dung of a hawk fed wholly on flesh was chiefly uric acid, and the evacuation of a gannet, feeding solely on fish, consisted altogether of urine; for the only solid parts were uric acid. "It seems," (our author concludes) "deserving of inquiry, what changes might be produced in the urine of any one animal by such alterations of its diet as its constitution would permit; for, as far as any inference can be drawn from these varieties, which naturally occur, it would appear that persons subject to calculi consisting of uric acid, as well as gouty persons, in whom there is always a redundance of the same matter, have much reason to prefer vegetable diet; but that the preference usually given to fish above other kinds of animal food, is probably erroneous.

The mention of Dr. Woollaston's paper naturally leads us to reflect on the important services which this truly philosophical inquirer has formerly rendered to the branch of science now under consideration. It was his important discovery of the nature of gouty concretions, which first ascertained the analogy long suspected between gout and stone; and, besides giving the first analysis of those concretions which had ever been experimentally attempted, he also first (with the able assistance of Mr. Tenant, a name far too well known to need the humble tribute of our admiration) investigated the composition of the calculi, consisting of neutral salts. The discovery of Scheele, perhaps his most important discovery, brought to light the nature of uric calculi; and, at the same time, made us acquainted with a new acid. The existence of this acid was suspected in gouty concretions; and it was generally imagined that they consisted of the acid alone, or combined with animal matter only. Dr. Woollaston showed by experiment that they are composed of uric acid and soda. Until he carried his inquiries into the other classes of calculi, it was only by conjecture or vague analogies that their constitution was known; but to him we owe as perfect an acquaintance with their component parts, as Scheele had left us of the uric calculus. Having thus completed our analysis of the calculi formerly known, he has, in his present communication, brought to light both a new calculus, and a new body. His former paper was published in the Philosophical Transactions for 1797; and the one now before us forms an important appendix to it. It is not within our province to take any further notice of the first tract; but we could not conclude this article without recalling to the recollection of the scientific reader, and reminding him of his great obligations to Dr. Woollaston.

Voltaic Apparatus.

Mr. John George Children laid before the Royal Soctety an account of some experiments on the most advantageous method of constructing a Voltaic apparatus.

His battery consisted of upwards of ninety-two thousand square inches in surface, each plate being four feet by two; and it was charged with a mixture of three parts of fuming nitrous, and one part of sulphuric acid, diluted with thirty parts of water. With this battery he fused completely eighteen inches of platina wire, one-thirtieth of an inch in diameter, in twenty seconds, and ten inches of iron wire one-seventieth of an inch thick: charcoal was burnt, giving out an intense brilliancy. But on imperfect conductors it had not the slightest effect; and on the human body it was hardly perceptible; and it had scarcely any effect on the gold leaves of an electrometer. But with a second battery, consisting of two hundred pair of plates each, about two inches square, placed in half-pint pots of common queen's-ware, and rendered active by some of the liquor used in exciting the large battery, to which was added a small portion of fresh sulphuric acid, he readily decomposed potash and barytes: in that state it produced the metallization of ammonia with great facility; it ignited charcoal vividly; it caused great divergence in the leaves of an electrometer; and it gave a vivid spark after being in action three hours. Hence it is inferred, that Mr. Davy's theory is accurate, viz. "that the intensity increases with the number of plates, but that the quantity of electricity increases with their size." Thus the platina wire being a perfect conductor, and not liable to be oxydated, presents no obstacle to the free passage of the electricities through it; which, from the immense quantities given out from so large a surface, evolve, on their mutual annihilation, heat sufficient to raise the temperature of the platina to the point of fusion. With the iron wire the effect was different, on account of the low intensity of the electricity, (sufficiently proved by its not causing any divergence of the gold leaves of the electrometer) which being opposed in its passage by the thin coat of oxide formed on the iron wire at the moment the circuit is completed, a very small portion only of it is transmitted through the wire. To the same want of intensity is to be attributed the inability of the large battery to decompose the barytes, and its weak action on imperfect conductors in general. The small battery, on the contrary, exerts great power on imperfect conductors, decomposing them readily; although its whole surface is more than thirty times less than that of the great battery: but in point of number of plates, it consists of nearly ten times as many as the large one. The long continued action of the small battery shows the utility of having cells of a sufficient capacity to hold a large quantity of liqour; and in large combinations, a certain distance between each pair of plates is absolutely necessary to prevent spontaneous discharges which will otherwise ensue. Mr. Children also made experiments to ascertain whether there was any striking distance in the Voltaic spark; and he found that with twelve hundred and fifty plates, four square inches surface each, the striking distance was about one-fiftieth of an inch in length, and he assumes, that by increasing the number of the plates, the striking distance will also be increased.

"The absolute effect," he says, "of a Voltaic apparatus, seems to be in the compound ratio of the number and size of the plates; the intensity of the electricity being as the former, the quantity given out as the latter; consequently regard must be had in its construction to the purposes for which it is designed. For experiments on perfect conductors very large plates are to be preferred, a small number of which will be sufficient; but where the resistance of imperfect conductors is to be overcome, the combination must be great, but the size of the plates may be small; but if quantity and intensity be both required, then a large number of large plates will be necessary. For general purposes, four inches square will be found to be the most convenient size." [*Monthly Magazine for* 1810.

Reflective Goniometer.

Dr. Woollaston has lately given an account and description of a reflective Goniometer, to be applied to the measuring of angles and crystals. By this instrument, in most cases, the inclination of surfaces may be measured as exactly as is wanted for common purposes, and when the surfaces are sufficiently smooth to reflect a distinct image of distant objects, the position of faces only one-fiftieth of an inch in breadth may be determined with as much precision as those of any larger crystals. For this purpose, the ray of light reflected from the surface is employed as a radius, instead of the surface itself; and for a radius of one-fiftieth of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches, or by a second mode, substitute the distance of objects seen at a hundred or more yards from us. The instrument consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar. This axle being perforated, admits the passage of a smaller axle through it, to which any crystal of moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion. The position of the crystal is first adjusted, so that by turning the smaller axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye. The circle is then set to zero, or 180°, by an index attached to the pillar that supports it. The small axle is then turned, till the farther surface reflects the light of a candle, &c. to the eye; and, lastly, the circle is turned by its larger axle, till the second surface reflects the same light. This second surface is thus ascertained to be in the same position as the former surface had been. The angle through which the circle has moved is the supplement to the inclination of the surfaces; but as the graduations on its margin are numbered accordingly in an inverted order, the angle is correctly shown by the index without computation. By this instrument a perfectly clean and uniform fracture is not necessary; for since all those small portions of a shattered surface, that are parallel to one another, glisten at once with the same light, the angle of an irregular fracture may be determined nearly as well as when the reflecting fragments are actually in the same plane. The inventor of this goniometer thinks the accuracy of it to be such, that a circle of moderate dimensions, with a vernier adapted to it, will probably afford corrections to many former observations. He adds, that he has already remarked one instance of a mistake that prevails respecting the common carbonate of lime, which he particularly mentions, because this substance is very likely to be employed as a test of the correctness of such a goniometer, by any one who is not convinced of its accuracy from a distinct conception of the principles of its construction. The inclination of the surfaces of a primitive crystal of carbonate of lime, is stated to be 101° 28′ 40″, a result deduced from the supposed position of its axis, at an angle of 45° with each of its surfaces. Dr. Woollaston contends that the angle is not 45° exactly, but 45° 20′; for he finds the inclination of the surfaces to each other is nearly, if not accurately, 105°, as it was formerly determined to be by Huygens; and since the measure of the superficial angle, given by Sir Isaac Newton, corresponds with this determination of Huygens, his evidence may be considered as a confirmation of the same result. [*Monthly Magazine.*

Hints on the subject of Animal Secretions.

Mr. Home has communicated to the Royal Society some hints on the subject of animal secretions, with a view of throwing new light on animal chemistry. The discoveries of Mr. Davy suggested to Mr. Home the idea, that the animal secretions may be produced by chemical changes effected by the power of electricity. The voltaic battery, he observes, is met with in the torpedo and electric eel, a circumstance that furnishes two important facts: one, that a voltaic battery can be formed in a living animal; the other, that nerves are essentially necessary for its management; for in these fish the nerves connected with the electrical organs, exceed those that go to all the other parts of the fish, in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a voltaic battery of high power; but their structure appears to adapt them to receive, and preserve a small electrical power. That the nerves arranged with muscles, so as to form a voltaic battery, have a power of accumulating and communicating electricity, is proved by the well known experiments of the frog. There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves. With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. Brande, at the suggestion of Mr. Davy, made some experiments, first upon blood recently drawn from the arm, and then upon a deer, in order to obtain the blood in a perfectly fluid state. Finding, however, the coagulation of the blood an insurmountable obstacle to the long continued electrical action, the serum only was employed. In one experiment coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These effects were produced by a high electrical power upon serum. With a lower power there was no appearance of coagulation at either pole; in five minutes the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length, was seen floating in the fluid, and adhering to the same wire. By these, and other experiments, it was ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form, with a less degree it is separated in a fluid form. From these facts the following queries are proposed: 1. That such a decomposition of the blood, by electricity, may be as near an approach to secretion as could be expected to be produced by the artificial means, at present in our power. 2. That a weaker power of electricity than any that can be kept up by art may be capable of separating from the blood the different parts of which it is composed, and forming new combinations of the parts so separated. 3. That the structure of the nerves may fit them to have a low electrical power; and as low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts. 4. That the discovery of an electrical power which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since albumen is the principal material of which animal bodies are composed. 5. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity. 6. As albumen becomes coagulated by the effect of a power too low to affect the most delicate electrometer, may it not occasionally be employed as a chemical test of electricity, while the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions? [*Monthly Magazine.*

Mr. Penwarne's Patent for an Invention, or Process, for giving Statues, or other ornamental Works in Plaster, an appearance nearly resembling Marble.

The principle of this invention is to impregnate the said plaster of Paris with sulphate of alumine, (alum) which is done in the following manner. A solution of alum in water is prepared in the proportion of one part of alum to three of water. The liquor is then made to boil, or is heated to a degree of temperature sufficient to dissolve the alum. The plaister-cast, previously dried and finished off, is then immersed in the above solution, and suffered to remain therein from fifteen minutes to half an hour; it is then taken out and suspended over the vessel containing the solution, and when cooled some of the solution is dipped up and thrown over it, or applied to it by means of a sponge, or linen cloth, and which is continued till the alum forms a fine crystallization over its surface, of a due degree of thickness; when it is sufficiently dry it may be brought to a proper degree of smoothness, or polish, by means of sand-paper, or glass-paper, and finished by being rubbed with a fine linen cloth, slightly moistened with clean water. The vessels made use of in this business are made of wood, heated by steam, introduced through a leaden pipe, from a boiler.

Hitherto the plaster-cast has been valued for the facility it afforded of producing cheap, correct, and faithful copies of the finest works of the chisel; but it possessed no other advantages; its colour was unfavourable, it was liable to soil from the touch, and from dust, and its fragility and softness rendered it next to impossible to clean it. The present invention is said completely to obviate these disadvantages, and the subject of it may be regarded as an intermediate step between the plaster-cast, and the expensive marble. It possesses the beautiful whiteness and transparency of the finest statuary marble, and at the same time scarcely yielding to it in hardness and durability. It is not affected by the moisture of the dampest apartment; is less liable to soil, and as easily cleaned as marble.

[*Monthly Magazine.*

Oil of Poppy Seeds.

Considerable quantities of poppy seeds have lately been bought up in different parts of the country, and the expressed oil from them sold at the price of Florence oil. Major Cochrane, of Haddington, was the first person who stated the advantages arising from the cultivation of poppies, and that seven ounces of fine salid oil were furnished by expression from two pounds of the seed. [*Ibid.*

Large Voltaic Apparatus.

At the concluding lecture for the season at the Royal Institution, the large voltaic apparatus, consisting of 2000 double plates, four inches square, was put in action for the first time. The effects of this combination, the largest that has been constructed, were of a very brilliant kind. The spark, the light of which was so intense as to resemble that of the sun, struck through some lines of air, and produced a discharge through heated air nearly three inches in length, and of a dazzling splendour. Several bodies, which had not been fused before, were fused by this flame: the new metals discovered by Mr. Tennant, iridium, and the alloy of iridium, and osmium. Zircon and alumine were likewise fused; charcoal was made to evaporate, and plumbago appeared to fuse in vacuo. Charcoal was ignited to intense whiteness by it in oxy-muriatic gas, and volatilized in it, but without being decomposed. A large Leyden battery, containing twenty-four coated jars, was charged by a momentary contact of the wires, to a degree that required from twenty to thirty turns of Nairne's machine of eight inches diameter. All the electical phenomena of the passage of electricity to a distance, the discharge through a Torricellian vacuum, the attractions and repulsions of light bodies, were demonstrated in a distinct way by means of this apparatus. It is hoped that the application of so powerful an instrument, and such easy methods of producing the most intense heat, will lead to some new facts in analytical science. [*Ibid.*

On the Combination of Acids with Animal and Vegetable Substances. By M. Thenard.

My researches upon the nitric, muriatic, and acetic ethers, and upon those obtained by treating alcohol with muriate of tin and oxy-muriatic acid,* have naturally led me to examine if it was possible to form them with the other acids. I have tried the action of these acids upon alcohol; and it was in making and varying these experiments I arrived at the singular result which I have had the honour to communicate to the Institute; a discovery, that when the vegetable acids are pure, none of them, excepting the acetic, combine with alcohol with the loss of their acid properties; but on the contrary, when mixed with a mineral acid capable of condensing alcohol strongly, they all form with that body a combination in which their acid properties disappear, without the mineral acid taking any part in this combination.

It is evident then, that whatever may be the mode of the combination of alcohol with a vegetable or mineral acid, the alcohol produces in these compounds the effect of a true saline base.

Now, the question is, whether the property of combining with acids, and also of neutralizing them, does not belong to all animal and vegetable substances. It is very possible that this is the case; for, since alcohol possesses this property, all these substances may also possess it. It was with a view to solve this question that the following experiments were made.

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* Vol. i. des Memoires d'Arcueil.

I passed over 300 grammes of alcohol, oxy-muriatic acid gas made from a mixture of 1758 grammes of muriate of soda, of 450 grammes of black oxide of manganese, of 800 of concentrated sulphuric acid, and of 800 grammes of water.

Almost all the acid and the principal part of the alcohol were mutually decomposed, and either generated or liberated a large quantity of water, of matter having an oily appearance, of muriatic acid, and a small quantity of carbonic acid, and a substance abounding in carbon; a result that agrees with what has already been published either by M. Berthollet in the Memoirs of the Academy, or by myself in the first volume of the Memoirs d'Arcueil. All these products have been separated with care, as I have noticed in those memoirs; one only has been subjected to a new examination, and that is the oily matter.

When carefully purified by water and potash, it has the following properties, some of which have been already observed in the memoirs I shall quote. It does not redden turnsole paper; it is white; it has a cool taste similar to that of mint, and a particular but not ethereated smell; it is heavier, yet less volatile, than water; it is very soluble in alcohol, but very slightly in water. It is volatilized by distillation with nitric acid, and partly decomposed; but the products of this decomposition vary according to the strength of the acid used. If the nitric acid be weak, much muriatic acid is produced, and little oxy-muriatic; if, on the contrary, the acid be concentrated, little muriatic acid is procured, but much oxy-muriatic acid: of course this substance contains a very considerable quantity of muriatic acid. In the same manner, when it is passed through a red-hot iron tube a large quantity of acid is disengaged. Yet it is decomposed but very slowly by the strongest alkalis, even when dissolved with them in alcohol: hence the conclusion must be drawn, that the muriatic acid it contains is intimately combined with another substance. I have not yet succeeded in discovering what this substance is, because I have not been able to separate it from every thing else. Whatever that may be, it is certain that it is capable, like the alkalis, of neutralizing acids: and it may be presumed that it contains a large quantity of carbon, since in the decomposition of alcohol and oxy-muriatic acid much water and very little carbonic acid are produced.

But of all the vegetable substances, I am acquainted with none that possess the property of uniting themselves to acids, in a more eminent degree than some of the essential oils, perhaps even all of them enjoy this property. That of turpentine absorbs nearly one-third its weight of muriatic acid gas, and becomes converted, with the emission of much heat, into an almost entire crystalline substance. Kind some years since discovered it; its nature was afterwards studied by Tromsdorff and some French philosophers, and last of all by Gehlen. All these chemists, except Gehlen, have considered it as an artificial camphor, because it had the smell, volatility, lustre, whiteness, and many other properties of natural camphor; and comparing the action of muriatic acid on oil of turpentine, with that of sulphuric acid on vegetable substances, they have conceived that the transformation of this oil into camphor is solely to be attributed to the loss of oxygen, and hydrogen being abstracted in sufficient quantity by the muriatic acid to form water, and to a slight separation of carbon at the same time; in fact, that the artificial camphor, with a little more carbon, and a certain quantity of oxygen and hydrogen, in the proportions to constitute water, would return to an essential oil.

Gehlen rejected this theory for good reasons.* Having observed that in passing muriatic acid through the essential oil of turpentine, no gas was disengaged; that only a certain portion of the oil was converted into white crystals having the appearance of camphor, and that the remainder appeared as a brownish-black liquid of great acidity; that the crystals too as well as the liquid contained muriatic acid in intimate combination, &c. he has concluded, that in this operation the essential oil of turpentine is decomposed, that the greatest part of its hydrogen combines with a small quantity of its carbon, and with a certain portion of muriatic acid, to form concrete camphor, whilst the other principles of the oil combine with the remaining portion of muriatic acid to form the brownish-black liquid; therefore that this liquid contains more carbon and less hydrogen than the camphoric concretion; and that, in the formation of both, the muriatic acid acts only by the tendency it has of uniting to them, and not, as the chemists above quoted say, by forming a certain quantity of water at the expense of the principles of the essential oil.

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* Vide Journal de Gehlen, tome vi. p. 458.

I repeated and varied with great care the experiments on the essence of turpentine and muriatic acid, made by Kind, Tromsdorff, &c. and above all by Gehlen. I obtained the following results: 100 grammes of essence of turpentine purified by distillation, and plunged into a freezing mixture of ice and salt, absorbed 30 grammes of muriatic acid gas; no gas but muriatic escapes, which perhaps was not entirely absorbed at first, or passed through the liquid when the operation was nearly finished. The essential oil is converted into a soft and crystalline mass, from which, during its desiccation for three days, may be separated 20 grammes of a liquid that contains many crystals and nearly 100 grammes of a white substance, granular, crystalline, volatile, and having a strong smell of camphor. This substance quickly loses, by exposure to the air, the property which it at first has of reddening turnsole paper, and afterwards only very minute quantities of acid can be separated from it, even by warm alkaline solutions. Yet when it is sublimed, the vessels in which the operation is made become strongly acid; and when, instead of subliming it, it is passed through a red-hot iron tube, a greater quantity of acid is still abstracted from it. Lastly, when decomposed both by concentrated and weak nitric acid, a large quantity of oxy-muriatic acid is produced in the first instance, and in the second much muriatic acid. Relative to that portion of liquid derived from the crystalline mass, the weight of which was 20 grammes, it was white, and diffused acid vapours, but ceased after an exposure of some days to the atmosphere, and was no longer acid; it crystallized in mass some degrees below zero, and preserved an intimate union with much muriatic acid.

Thus all my observations, except one only, agree with those of Gehlen. That in which we do not coincide is relative to the liquid product of the operation. Gehlen obtained it of a brownish-black, and I always obtained it white. This difference, I think, depends on Gehlen having used the essence known in commerce, which contains, unless distilled with great care, a resin, and has then the property of blackening and forming a large uncrystallizable residue. Neither do I draw from my observations the same theoretical inferences as this chemist does from his. I think that the essential oil of turpentine is not decomposed by muriatic acid, and consequently that these two bodies combined together in certain proportions form concrete camphor; probably the small portion of liquid product obtained by him is of the same formation; besides, it is not improbable that the difference existing between the camphoric concretion and the liquid product, depends on a small quantity of an ethereal oil mixed with the essence, and capable of forming with muriatic acid a liquid product. Lastly, what induces me to believe that the muriatic acid does not decompose the essence, but, on the contrary, combines with it, is, that this acid evidently combines with all the principles of alcohol without affording many different products, and that alcohol is a hydrogenous body as well as the essential oil of turpentine. Yet whatever it may be, nothing is more certain than that this combination has the greatest resemblance to native camphor.

Perhaps the camphor which is extracted from the essential oils of plants, particularly from that of lavender, is a combination of essence and acid alone; perhaps, too, the camphor of commerce, or the camphor of the laurel, which does not appear to contain any mineral acid, (for none can be obtained from it by destructive distillation) is formed of an essential oil and vegetable acid alone. These views, as they are founded on facts, at least deserve some attention, and, if confirmed by experience, may probably be of great advantage.

I have likewise formed the combinations of the essence of lemon and of lavender with muriatic acid. 32 grammes of the essence of lavender require 22 of acid; the resulting compound is blackish, acid and liquid. 26 grammes of the essence of lemon absorb 22 grammes of acid; the essence is of a brownish hue, and solid. Neither of these essences had been purified; but both were used in the state they are generally found in commerce.

The essential are not the only oils capable of combining with acids. It has long been remarked that the fat oils form with different acids peculiar kinds of soap. The sulphuric acid, when concentrated, has this effect upon all of them; it produces with the oil of olives in particular a soapy matter, of a thick and greenish appearance, which gradually acquires consistence; when washed with water it becomes white; it is acid, but less so than if the acid was uncombined, and it may be brought into the neutral state by a proper quantity of potash.

Tannin besides, which is a vegetable matter, contracts as is well known, a strong union with acids; for the concentrated sulphuric acid suddenly precipitates it from its solution in water, and I am convinced that however often the precipitate may be washed, it is always acid. Nature itself has offered us a compound of this species. In fact, I have treated 10 grammes of gallnuts with 2 litres of boiling water, and although the residue had been washed with 2 litres more of very warm water, it still strongly reddened tincture of turnsole: in the gallnuts, therefore, the gallic acid is retained by a true affinity. But as the quantity of gallic acid abstracted by a spontaneous decomposition far exceeds the quantity of free acid existing in its natural state, we must conclude that the greatest part of the acid is certainly neutralized by the tannin. It is observed too, that if a decoction of gallnuts be allowed to follow its own course, as the tannin is destroyed, the liquor becomes more and more acid. One may, indeed, attribute this phenomenon to the transformation of the tannin itself into gallic acid; but numberless observations authorize us to believe that this is not the case.

1. That vegetable substances during their decomposition form no other acid but the acetic.

2. That gallnuts evidently contain abundance of gallic acid, and that, this acid having a strong affinity for tannin, the two bodies should necessarily neutralize each other in part.

3. Because there are many remarkable instances of the power of some vegetable substances to neutralize acids.

4. Lastly, that tannin precipitated from a decoction of gallnuts by an alkali or carbonate of ammonia, contains, according to my experiments, a good deal of gallic acid as well as a small portion of saline base. The proof I have of this is, that it produces with almost all the metallic solutions the same effects as gallic acid, or gallate of potash. Thus it gives red precipitates with solutions of the peroxides of mercury, blackish grey with solutions of peroxides of iron,

a blue one to those less oxidized, the colour of wine lees *(lie de vin)* to those least oxidized. It follows, then, that we are not yet acquainted with the pure tannin of gallnuts, and of most other substances, and consequently that we know not how to act with them on metallic solutions, &c. Inquiries which have this object in view cannot but be interesting; and I propose to pursue them, in as much as they will on some future day allow me to bring forward the question I am now treating of.

If, after having examined the vegetable substances which readily combine with acids, a search is made among animal substances endowed with the same properties, five will evidently occur: curdy matter, albumen, *picromel*, gelatin, and urea. This property is generally found in the curdy matter, as it is known that acids coagulate milk, that the coagulum contains an acid, and that this acid is even sensible to turnsole paper. Of course the affinity between those two bodies is well marked; yet it is not sufficiently strong to destroy all doubts to the contrary; for I am satisfied that the excess, and perhaps also the whole of the acid may be abstracted by means of a large quantity of water.

If water alone be sufficient to take the acid from the curdy matter, it fails to produce the same effect when the acid is united to albumen; for the washings may be continued to an unlimited extent, and there will be always found acid undissolved.

We must conclude, therefore, that this animal substance exerts on acids a more powerful attraction than the curdy matter; but this action varies in itself, according to the concentrated state of the acid. As the acid is saturated with water, the results are combinations more or less insoluble, and which again dissolve, the acid being saturated with ammonia, or with either of the other alkalis, and in which albumen is no wise altered. If, on the contrary, the acid is very strong and concentrated, the precipitates formed will always be acid, but will contain albumen in the state in which it exists when concreted by fire: this has appeared in treating the precipitates with a weak solution of ammonia. In the first instance, the solution will be gradually formed, and complete: in the second, the acid alone will be taken up, and the albumen will remain undissolved in the form of a thick *magma*.

The combination of picromel with the acids took place with the same facility as with the preceding. Almost as soon as these substances come in contact, an acid precipitate is formed, which redissolves when neutralized. It is principally with the sulphuric, nitric, and muriatic acids that picromel forms slightly soluble compounds.

The concentrated nitric acid is the only one, according to Messrs. Fourcroy and Vauquelin, that precipitates urea from its solution in water; yet it is probable that the other acids are capable of combining with it; and if they do not precipitate it, it is because the compounds are soluble in water. I have observed that these combinations never form when the temperature is below 40 or 50, particularly with the sulphuric and muriatic acids; and that otherwise there is a disengagement of carbonic acid, and a production of a large quantity of ammonia.

Lastly, gelatin itself may unite with some acids, and principally with the oxy-muriatic acid. To effect this combination it is necessary to pass the gaseous acid through a solution of gelatin. In this operation the solution gradually becomes turbid and precipitates flakes, which unite together in the form of pearly filaments very elastic and flexible. These flakes have been considered by M. Bouillon-Lagrange, in a memoir upon the gizzard of birds, as the oxygenated gelatin:* but it is a mistake; they are really formed of gelatin slightly altered, and of the muriatic and oxy-muriatic acids. Their striking characteristics are, that they are insipid, insoluble both in water and alcohol, not liable to putrefy, slightly acid: though a sufficiently large portion of acid enters their composition to emit spontaneously for several days the oxy-muriatic acid gas, much more may be disengaged by means of heat: lastly, they contain so much acid as to be soluble in alkalis, and to form with them muriates.

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* Annales de Chimie, tom. vii. p. 24.

Thus we have examined five vegetable matters, and six animal matters, capable of an intimate union with acids. Three of the former, viz. alcohol, essential oil of turpentine, and a substance abounding in carbon, and produced from alcohol decomposed by oxy-muriatic acid, neutralize acids equally as well as the strongest alkalis. The last seven form with these acids combinations, which are themselves acid, like the metallic and most of the earthy salts.

Without doubt we shall in time be able to combine all other vegetable and animal substances with acids, and the experiments related allow even at present the inference to be drawn; for, if the combination cannot be made directly, no proof appears in this against what I advance. Does not alcohol, which in its ordinary state has not the property of neutralizing the vegetable acids, acquire it by the presence of a mineral acid? When these substances are placed in different circumstances probably the one best adapted to unite them with the acids will be found. These researches are laborious indeed; but they are useful and important, as tending to introduce us to the knowledge of a long compound series of a particular order; a knowledge which in itself must throw great light upon the analysis of vegetable and animal substances. In fact, is it not possible that we may find in organic bodies compounds of this kind? Are not gallnuts an example? Is it not possible that acetic acid, which we procure by the distillation of animal and vegetable substances, may be contained ready formed in some? Amber, in particular, from which we get by distillation succinic acid, is it formed of an oily matter combined with succinic acid? The fatty substances which afford us sebacic acid, may they not also be in similar circumstances? Lastly, the bitter principle formed by treating animal substances with nitric acid, which acts as a very oxygenated body, although not acid; and the yellow acid, which, according to MM. Fourcroy and Vauquelin, is formed in the same operation, may they not be intimate combinations of an acid and of another matter? But it is above all things necessary, in the explanation of phenomena which pass before us, in the decomposition of vegetable and animal substances by acids, that a strict account should be kept of their tendency to unite with these bodies.

[*Medical and Physical Journal.*

A Memoir on the Physiology of the Egg. Read before the Linnean Society of London, by J. A. Paris, M. B. Physician to the Westminster Hospital. London, 1810.

This interesting and philosophical essay commences with a few cursory observations on the opinion, that all animal and vegetable productions proceed from eggs or seeds.

"The extensive range which the *ovipari* form in the scale of animated existence, renders the organization and developement of the egg a subject of great interest to the naturalist, whilst the hope of ascending to the source of vitality, by contemplating life at a period when the number and complication of its functions are the fewest, becomes a powerful inducement to the physiologist, to pursue the investigation; hence we find, that the philosophers of every age and nation have devoted much time and labour to this inquiry; but unfortunately for the earlier promotion of science, the influence of chemical powers in the scheme of animal life has but lately been duly appreciated; many beneficial results, however, have already attended this discovery, and the most exhausted topics of natural history have assumed new and unexpected aspects. The author, therefore, of this memoir, may reasonably hope to escape the censure which must otherwise have awaited the adventurer who could presume to beat the field, which has before been so ably explored by the indefatigable and united labours of Fabricius *ab aqua-pendente,* Harvey, Malpighi, Spallanzani, Hunter, and others equally illustrious.

"A powerful phalanx of philosophers maintain, with much plausibility, that the *egg** is the universal womb of nature, and that *oviparous* differ only from *viviparous* animals, by the latter breaking their bondage before they escape from the parent. Concerning the truth, however, of this opinion, which is comprehended in the popular aphorism, '*omnia ex ovo;*' or the success with which the eloquent Count de Buffon has levelled his shafts against the partisans of the ovular system,† I will leave to be decided by abler disputants. The observations which I beg to submit to your notice, do not involve either theory, but are connected only with those animals that are *oviparous,* in the common acceptation of the term—that is, *who deposit a germ to be developed by causes totally independent of parental influence* as such.

"Amongst the countless multitudes and varieties of animals, a very small proportion only produce living offspring; thus the immense tribes of birds, fishes, amphibious animals, and insects, with comparatively few exceptions,‡ propagate their species by the intervention of the egg; nor is this mode of generation either accidental or unimportant. Had the winged inhabitants of the air been *viviparous,* the burthen of gestation would have impeded the action of their wings, and so far increased their gravity, as to have rendered them incapable of the exertion of flight. The rigid and unpliant coverings of *crustaceous* animals, would oppose the expansion necessary for the growth of a fœtus; and it is evident, from the structure and habits of the tribe of serpents, that if they had been *viviparous,* their offspring must, from their tortuous flexions, and the friction arising from their progressive motion, have been constantly exposed to injury and destruction; and, lastly, the *multiparous** nature of insects and fishes at once convinces us how impossible it would be to engender them by any other mode than that which nature employs."

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* *Egg,* the word *ovum* seems to be derived from the Greek word *oiov, solitarium,* because (unlike most *uteri*) it produces but one offspring.

† The system of the ovarists has been preferred by Harvey, Steno, Malpighi, Valisnieri, Dulamel, Nuck, Littre, Swammerdam, Haller, Spallanzani, Bonnet, &c.

‡ Some fish are viviparous, E. G. *Muræna Anguilli,* or eel, *Blennius Vivaparous,* &c. Amongst the *Amphibia,* we may notice the *viper,* which brings forth its young alive, and hence probably derives its name, "*quod vivum pariat.*" Spallanzani considers also the production of frogs as rather of a viviparous than oviparous nature; this rudiment, however, of the future animal certainly partakes as much of the nature of an egg as of a fœtus, and may probably be considered as a connecting link between the two great classes. Insects likewise present us

As the author confines himself in this memoir to the physiology of a particular description of eggs, he proceeds to lay down his distinctions and definitions of the several component parts of the eggs of birds, and the progress of their formation.

"The eggs of oviparous animals admit of an evident division into two classes, which I shall name. 1. The Perfect, and, 2. The Imperfect. The former, which are deposited by the *Aves,* and some genera of *Amphibia,* are completely covered by a hard shell, or membrane, and receive no addi-

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with exceptions, and several whimsical varieties; the *Aphides,* for instance, lay eggs at the end of autumn, when the young produced from them in the spring are viviparous during the whole summer! The *Cocci* hatch their eggs before exclusion, and the young force their way through the abdomen of their mother; the *Onisci* carry their eggs in a particular receptacle, from which, in process of time, the young escape.

* Reaumur ascertained, that a single queen bee had laid, in the months of March and April, 12,000 eggs; and Lewenhoeck found that the *Musca carnaria* deposited 144 eggs, from which, in one month, were produced as many flies; so that supposing one half of these to be females, there would be, in the third month, 746,496 flies. The amazing fertility of fish may be illustrated by the *Gadus morhua,* or cod-fish, which will deposit among the rocks nine or ten millions of eggs; and again the parch, *Perca fluviatilis,* produces in April and May 281,000 ova!

tions after their exclusion; whilst the latter are deposited by most *Pisces,* and in general constitute a soft mass, not being protected by any external involucre. The observations contained in this memoir relate more particularly to the egg of birds; their history, however, comprises whatever is interesting or important in the germs of inferior animals.

" In order that I may be better able to form a systematic relation of those new facts I wish to introduce to your notice, I shall briefly relate the successive operations by which the egg is formed in the body of the animal; the necessity of a more diffuse description is superseded by the minute and valuable details which may be found in the writings of Harvey and Malpighi.

" The rudiments of the *ovum* are first visible in the *ovarium,* which, in fact, is nothing but a congeries of *vetelle,** or yolks, attached to the spine by a proper membrane; this repository is denominated, by Fabricius, the *vitellarium,* or *vitellorum racemus,* and may be considered as analogous to the *ovarium* of the *mammalia,* or to the *roe* of fishes. These *vitelli,* or *yolks,* generally vary in progression, from the size of a millet seed to that of an acorn; each according to its maturity, is successfully detached from the rest, whence it descends a tube, called (from its resemblance to a funnel) '*infundibilum,*' and arrives at the *uterus,* the internal surface of which is extended by spiral convolutions; here the albuminous fluids are secreted and transmitted to the egg, during its passage to the *fundus uteri* and *cloaca,* where it receives its external crust from the calcareous matter which is there deposited on it.

" The egg thus formed and completed, possesses every essential for its subsequent maturation, and requires only the emphatical agency of heat for its developement: this, in the different genera of animals, is conveyed by different media; in birds it is applied through incubation,* but in the *amphibia*, and other animals, the heat of whose bodies is inconsiderable and inefficient, the eggs are deposited in mud or sand, or exposed to the rays of the sun, by whose prolific influence myriads of beings are daily called into life and activity; or they are placed in other favourable situations, all of which are well known to the disciple of Linnæus. It is, however, worthy of remark, that the medium through which heat is applied, is suitably varied in the same genus in different climates; thus, in Senegal, the ostrich abandons her eggs to be hatched by the burning sands, whilst, in the more temperate and congenial regions of the Cape of Good Hope, like other birds, she is inclined to incubation.

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* *Vitellus,* deriv. a *vita,* because it contains the embryon.

"The different species of *Æstrus* will afford us an illustration of the variety of situations in which the *ova* of animals are deposited; the *œstrus bovis*, or gad-fly, deposits them under the skin on the backs of oxen; the *œstrus hæmorhoidalis* in the rectum of horses; and the *œstrus ovis* in the frontal sinus of sheep, &c. the excrescences also which are to be seen on cabbages, turnips, and other plants, always contain the egg of some insect. This faculty which animals possess of selecting an appropriate nidus for the reception of their ova is truly astonishing, and affords a most beautiful illustration of the wisdom and intelligence which nature displays in the adaptation of means for the accomplishment of her ends; whether, however, the animal is directed in its choice by a moral or physical feeling is not so evident; that is to say, whether it results from foresight, or from the blind obedience of a law of nature, which wisely connects the wants of the parent with the interests and welfare of the offspring; thus, for example, many animals, although termed *amphibious,* never resort to the water but to procreate their species; now are we to conclude that they repair thither because they know that this element alone can unfold their ova, or because, by so doing, they gratify some internal sensation, which renders their abode on dry ground unpleasant?"

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* There are also other animals which accelerate the evolution of their ova, by incubation; thus bees in a hive generate a considerable quantity of heat, without which their eggs would perish; and the *Testudo Mydas*, or common turtle, deposits her eggs in the sand, and incubates during the night.

A general account of the constituent parts of a *perfect* egg having been already given, this ingenious and diligent naturalist enters on the detail of its anatomy, if we may be allowed the expression.

"The parts of which the *egg* consists are, 1. *Vitellus,* or yolk, with its *capsule* and *cicatricula.* 2. *Albumina,* with their proper membranes. 3. *Chalazæ.* 4. *Folliculus aeris.* 5. *Common membranes.* 6. *Exterior involucrum,* or shell; to each of which we will successively direct our attention.

"The *vitellus,* or yolk, is the part formed *in vitellario,* and is a yellow fluid contained in a membranous capsule, in which a white circular disk* is discernible; this is named *cicatricula,* and is the speck of entity, the germ that is to be developed into the animal. 'In hujus gratiam,' says Malpighi, 'reliqua comproducta videntur.' We here then have arrived to the earliest stage in which we can detect the animal's existence, our imperfect faculties will not allow us to ascend farther, and yet, even now, the body is formed, as the experiments and observations of Malpighi† most clearly demonstrate. The yolk is surrounded by a more tenacious fluid, to which the name of *albumen,* or commonly the white, has been assigned; this may easily be divided into two separate and distinct portions, each of which is contained in a concentric membrane; they differ from each other considerably in specific gravity, and seem to answer different purposes in the economy of the egg; the consistence of that which is exterior is far less than the one which immediately envelops the yolk, and is consumed in the earlier periods of incubation, whilst the internal and more viscid *albumen* seems reserved for the latter stages, when the chick must require a greater proportion of *generative matter* than at any other period of its developement. Many of the ancient philosophers imagined that the chick was formed out of the yolk, and that the white afforded nutriment; such a theory must, however, be at once abandoned, when it is known that the *vitellus* suffers no change by incubation, except designed to administer support, until its digestive organs can gain sufficient powers to perform their functions, and the beak a degree of firmness adequate to withstand the hardness of its natural food. 'Ipsum animal,' says Pliny, 'ex albo liquore ovi corporatur, cibus ejus in luteo est;' doubtless, however, the *vitellus* answers also other purposes, or why should it be necessary to those birds* whose parents so sedulously supply them with nourishment?

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* Fabricius supposes it to be a vestige of the ruptured *peduncle,* or that portion of membrane by which each yolk is connected to the vitellarium, and Æmilius Parisanus contends that it is the semen of the male.

† Vid. Marcell; Malpighi "de form: pull; in ovo."

"At each end of the egg a white shining body is inserted into the capsule of the yolk, which extends into the *albumen*, in which it floats; these, from their resemblance in colour and transparency to *hail*, have gained the name of *chalazæ* or *grandines*, and, from having been formerly regarded as the sperm of the male bird, or *treddles.* Bellini supposes that they are composed of numerous canals, which open into the *amnios* or *cicatricula*, and send out their roots into the white, for the purpose of forming a communication between them; Munro, however, observes, that 'if they be canals they cannot have the least communication with the cavity in which the chick resides, at any time, or in any state of the egg, otherwise than as they are both adhering to the membrane of the *vitellus,* upon which, or within which, no particular fibres, no canals, are stretched to the *cicatricula.*' The *chalazæ,* says Harvey, appear to be the poles of the microcosm, and serve to connect the different parts of the egg, and to retain them in their due position; in addition to such an office, Denham ingeniously conjectures that as they divide the yolk into two distinct and unequal hemispheres, they will preserve the *cicatricula* (let the position of the egg be what it may) in the same situation; for since the *chalazæ* are specifically lighter than the white, the yolk is kept buoyant, and the *cicatricula,* as it resides in the smaller hemisphere, will be always uppermost; this, in my opinion, is the true theory of their use; for such a structure will not only preserve the *cicatricula* from the dangers of concussion, but, by regulating its distance from the source of heat, it will insure to it a more completely uniform temperature than could otherwise happen, and which is so essential to the evolution of the animal, that the smallest irregularity overthrows the nice balance of the different actions that are to mature it, and produces fatal effects. So solicitous, therefore, was nature to rescue the germ from the consequences of cold, that she has ordained other provisions, which seem equally as effective as the *chalazæ* for the preservation of its proper temperature; thus the *cicatricula* is on all sides surrounded by fluids, which are feeble conductors of caloric, these must therefore powerfully retard the escape of heat, and prevent the destructive chills which the occasional absence of the parent might otherwise induce. The eggs of many animals appear protected by an analogous apparatus; thus the ova of frogs, and other amphibia, are enveloped in spheres of mucilage, which the experiments of Spallanzani show to be essential, as he found when this gluten was removed, the egg immediately perished. It is an evident fact that those fishes who retain their vitality long after their removal from the water, as eel and tench.

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* Pigeons, for example, whose crops John Hunter ascertained to secrete a peculiar fluid during the breeding season, for the sustenance of their young.

have the power of secreting a slimy fluid, with which they envelop their bodies; whilst, on the contrary, those which, when drawn on shore, quickly die, as, for instance, mackerel, possess no such faculty, or, at least, only in a small degree; is it not, therefore, extremely probable, that this albuminous matter, by repressing evaporation, and preventing (like the fluids of the egg) the escape of heat by its non-conducting nature, is the principal cause of this peculiar tenacity of life?*

"The hen bird seems herself conscious of the mischief that would accrue from an irregular or decreased temperature; she is often seen to make use of her bill to push to the outer part of the nest those eggs that were nearest the middle, and to bring into the middle such as lay nearest the sides. The Egyptians,† however, by a nice adjustment of the temperature of their ovens, succeed in hatching a great proportion of the eggs subjected to artificial heat. The celebrated Reaumur introduced this method into France, and Sir James Hall has lately invented a regulating stove, by which an equable temperature for this purpose may be easily and advantageously procured. Sir Busick Harwood, the ingenious Professor of anatomy at Cambridge, has frequently attempted to develop the egg, by the heat of his hot bed, but has succeeded only in producing monsters; the evident result of an unsteady application of heat. It must, however, be remarked, that deviations from the correct temperature are injurious and fatal only in proportion to the degree of vital energy which the ovular embryon possesses;

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* Perhaps a prodigious accumulation of fat may also, sometimes, have a share in producing this effect; thus the *Silurus Glanis*, which is the fattest of all fresh water fishes, as it grows to the weight of 300lb. lives very long after being taken out of the water.

† The inhabitants of the single village Berme, amongst whom only this art is practised, give life, by means of their ovens, to two-thirds of the eggs intrusted to their care, amounting, in one season, which continues but for six months, to the astonishing sum total of 92,600,000. Corneille le Bruyn, tom. ii. has collected the observations of many travellers on this subject.

thus we learn, from the experiments of Spallanzani,* that the eggs of insects are better able to sustain the vicissitudes of temperature than those endued with more exalted vitality; thus it is that the eggs of cold blooded animals bear with impunity such an increase or decrease of temperature as is sufficient to destroy the animals themselves; for Spallanzani found *tadpoles* and *frogs* to perish at 110°, but their eggs only at 133°. If we pursue this inquiry, and, quitting the animal kingdom, descend into the scale of vegetable existence, where the energies of vitality are still more feeble and obscure, we shall find the same relative power of sustaining heat or cold between the plant and the seed, as we recognized between the animal and its egg.

(To be continued.)

University of the State of New-York.—College of Physicians and Surgeons.

(Circular.)

The several courses of Instruction will commence, as usual, in this Institution, on the first Monday in November. The Session will be opened by an Address from the President.

Since the last Winter important additions have been made to the Chemical Apparatus, a Laboratory has been built, and the Chemical Lecture-Room has been so altered, as perfectly to accommodate the Students attending Doctor Mac Neven, the Professor of that Science.

Such provision has been made by law for the Anatomical Department, that Dr. Smith, the Professor of Anatomy, will give a complete course of Demonstrations, and will also illustrate his Surgical Lectures, by performing all the operations, practicable upon the healthy subject, in the presence of his class.

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* Spallanzani. Tracts on the Nature of Animals and Vegetables.

Dr. Hosack will Lecture on the Theory and Practice of Medicine, and a full and complete course will also be given on Obstetricks, and the Diseases of Women and Children.

The above series of Lectures constitute the Winter Session; the Spring Term, which commences on the third Monday in April, consists of Lectures on Natural History, and on Botany; the former by Dr. Mitchill, the latter by Dr. Hosack.

The Lectures on Natural History are illustrated by a large collection of Mineralogical and Geological Specimens; and an elegant Museum, to which the Students can have access, affords every facility for the acquisition of zoological knowledge.

The Botanic Garden having been purchased by the State, and placed under the direction of the College, the Students of Botany will have an opportunity of visiting that establishment whenever they think proper, and of examining the many rare and valuable plants which it contains.

The various courses of Instruction delivered in the College, are so arranged, as to form a complete System of Medical Education; and the Trustees and Professors confidently promise, that no exertion on their part shall be wanting to fulfil the just expectations and liberal views of their Patrons, the Legislature and the Regents.

The Professors are bound to examine all Applicants for Degrees, and to report to the Regents such as they approve, on or before the first day of March in every year. The Diplomas are then granted, and the Degrees are conferred in this City, on the first Tuesday in May, the Candidate having previously delivered to the Professors, and defended at a public Examination, a Thesis on some subject connected with Medicine. The Thesis may be printed at the discretion of the Graduate, with the approbation of three of the Professors.

By order of the College,
SAMUEL BARD, M. D. President.
JOHN W. FRANCIS, M. D. Register.

New-York, June 8, 1811.

New Publications.

Mr. George Hill, of Baltimore, has just published the first number of "*The Baltimore Medical and Philosophical Lycæum. By Nathaniel Potter, M. D. Professor of the Theory and Practice of Physic in the College of Medicine of Maryland.*" This work is to be published in quarterly numbers.

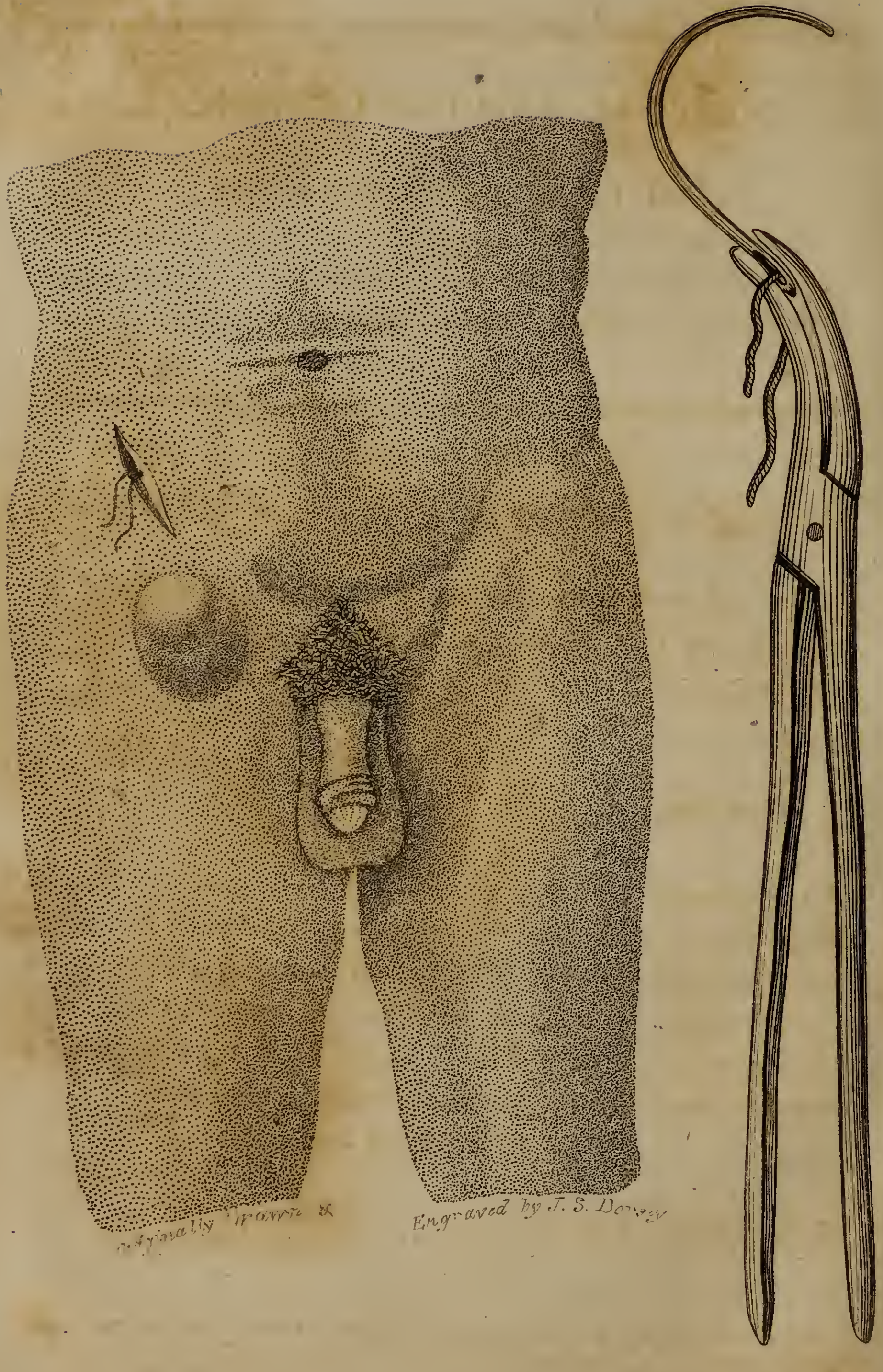
T. & J. Swords have in the press, and will publish in the course of a few days, "*Observations on the Climate in different Parts of America, compared with the Climate in corresponding Parts of the other Continent. To which are added, Remarks on the different Complexions of the Human Race; with some Account of the Aborigines of America: being an introductory Discourse to the History of North-Carolina. By Hugh Williamson, M. D. and LL. D. Member of the Holland Society of Sciences, of the Society of Arts and Sciences of Utrecht, of the American Philosophical Society, &c.*"

A. Finley, of Philadelphia, has just published the third number of "*The Electick Repertory, and Medical and Philosophical Journal*; and he has in the press, and will speedily publish, "*Observations on some of the principal Diseases of the Rectum and Anus, particularly Stricture of the Rectum, the hæmorrhoidal Excrescence, and the Fistula in Ano. By Thomas Copeland, Fellow of the College of Surgeons, &c. &c.*"

Dr. David Hosack has recently published, from the press of T. & J. Swords, "*Hortus Elginensis; or a Catalogue of Plants, Indigenous and Exotic, cultivated in the Elgin Botanic Garden, in the Vicinity of the City of New-York.*"

Dr. Hosack has also recently published, from the press of C. S. Van Winkle, "*A Statement of Facts relative to the Establishment and Progress of the Elgin Botanic Garden, and the subsequent Disposal of the same to the State of New-York.*"

Messrs. Bradford and Inskeep, of Philadelphia, and Inskeep and Bradford, of New-York, have recently published, in one octavo volume, "*Sixteen Introductory Lectures to Courses of Lectures upon the Institutes and Practice of Medicine, with a Syllabus of the latter. To which are added, two Lectures upon the Pleasures of the Senses and of the Mind; with an Inquiry into their proximate Cause. Delivered in the University of Pennsylvania, by Benjamin Rush, M. D. Professor of the Institutes and Practice of Medicine in the said University.*"



INGUINAL ANEURISM.

MEDICAL AND PHILOSOPHICAL

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For the Medical and Philosophical Journal and Review.

A CASE *of* WOUNDED ARTERY: *Communicated to* J. A. SMITH, *by* J. S. DORSEY, *M. D. and adjunct Professor of Surgery in the University of Pennsylvania.*

Philadelphia, 23d Dec. 1811.

DEAR SIR,

IN compliance with my promise, I have the pleasure to subjoin the case of aneurism which I consider a decisive* proof, that *wounded arteries may heal without an obliteration of their cavity.* I fully concur with you in the opinion that such a termination is extremely rare; but those writers who pronounce the thing impossible are surely mistaken.

Very respectfully I remain, &c.

J. S. DORSEY.

J. A. SMITH, Esq. &c. &c.

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* We cannot say that we entirely agree with Dr. Dorsey, that the case is *absolutely* "decisive," "that wounded arteries sometimes heal without an obliteration of their cavity." We admit that the authority of Dr. Physick is of such weight, that we do not exactly know how to dissent from it; but nothing short of demonstration by dissection would perfectly satisfy us of so extraordinary a fact as the one which this case tends to establish. *Edit.*

In consequence of a request from Dr. Physick, Dr. James Mease has obligingly furnished the following account of his case, which is extracted from a letter to Dr. Physick, dated Philadelphia, 21st Nov. 1811.

" On the 20th of September, 1795, while Physician of the port of Philadelphia, and residing at the old Lazaretto, on State-Island, I was attacked by a violent fever. My duties in visiting the shipping, and in attending the sick in the Hospital, were performed, during my sickness, by a young medical gentleman, sent down by the Board of Health. On his arrival I employed him to bleed me. He used a new lancet with broad shoulders, and plunged it deep in the barilic vein of the right arm, making a large orifice. The blood at first appeared very dark, for I had been in high health, and flowed as it does from veins not affected by the proximity of arteries; but as nearly as I can recollect, in about ten or twelve seconds it changed to a *florid complexion,* and was *discharged in jets.* Having kept my eye constantly upon the orifice, I instantly perceived the mischief that had been done; my father, who held the bason, also noticed the change in the colour of the blood. I lost about twelve or fourteen ounces of blood, and when my arm was bound up, and I had laid it across my breast, I plainly felt the pulsation of the blood while flowing from the artery into the vein, and perceived the thrilling noise from the same cause whenever I put my head under the bed-clothes. The external wound healed in the usual time, but I kept a slight compress upon the part for some days afterwards. I was bled five or six times during my illness in the other veins of the same arm, and in those of the left arm. On the 2d of October I resumed my duties.

" I was careful to use my arm as little as possible, for besides the continuance of the pulsation there was a swelling, about the size of a *small* filbert, in the injured part; but about five or six weeks after the accident I imprudently exercised it for half an hour, when the part swelled to the size of a common filbert, became painful, and looked red. The pain and redness, however, soon subsided, but the swelling did not diminish for upwards of two months; and when I came to town for the winter, you well remember my showing my arm, and telling you that I feared I should be obliged to trouble you in the course of the winter. To my great satisfaction, however, the tumour gradually subsided, and before the spring the arterial and venous blood flowed in the usual channels."

Dr. Physick having carefully examined the arm, was perfectly convinced of the existence of varicose aneurism, and did not entertain the feintest hope of a spontaneous cure, and the patient himself was very agreeably surprised by the happy termination of the case. At this time, and ever since the cure, the artery has been pervious, and pulsates like that in the other arm at the elbow and wrist.

The following case so clearly shows the immense improvement which has taken place in Operative Surgery within a few years, that we cannot forbear laying it before our readers, although it has been already published. But a very short time ago and the patient who was the subject of the operation would have been left to his fate, without one effort to rescue him from destruction; whereas now he may long continue in the enjoyment of perfect health; nor is the case more honourable to the profession than creditable to the operator, whose dexterity and boldness deserve every commendation.

CASE *of* ANEURISM. *By* JOHN SING DORSEY, *M. D.*

ON the 15th of August, 1811, I was consulted by Alexander Patton, on account of a tumour in his right groin, The patient was a native of Aberdeenshire, in Scotland, aged about thirty years, the last ten of which he had passed in America, having followed the trade of a cooper; was accustomed to hard labour, and to athletic exercises, jumping, running, and the like. He was six feet in height, of a robust but not a corpulent habit.

Two years ago he perceived, for the first time, a small tumour in the right groin. Having never had the venereal, nor indeed any other disease, and not having met with any accident, he was at a loss to account for this appearance. From its commencement it throbbed with considerable violence. For a year and four months it increased very slowly; during the last eight months much more rapidly. In January it was no bigger than a walnut, in August its shortest diameter was four inches, its longest nearly five. It occasionally gave him severe pain, and at length incapacitated him from all labour. In June last he applied to Dr. Irwin, of Easton, the place of his residence, who instantly apprised him of the nature and importance of the complaint, and advised him to come to Philadelphia. He arrived here the 14th of August, and was admitted next day into the Pennsylvania hospital.

On examination, an aneurism was found, situated immediately below Poupart's ligament, forming a regular tumour in the groin, nearly hemispherical, with a kind of apex, where the skin appeared extremely thin, and discoloured as if by ecchymosis. The patient had used a good deal of exercise previously to his admission into the hospital, and had taken a drink of rum, in consequence of which his arterial system was greatly excited, and the tumour pulsated so violently that the bed clothes were bounced up with great force. He was confined to bed; was purged, and kept to a low diet. A consultation was called; and the surgeons of the house concurred in recommending the operation of tying the artery as high as practicable above the tumour. It was determined to perform the operation promptly, as the disease was progressing, and no benefit was to be expected from delay.

On Monday, 19th of August, at noon, in presence of Dr. Physick and Dr. Hartshorn, surgeons to the hospital, and a number of medical gentlemen, I proceeded to the operation. The patient having previously taken fifty drops of laudanum, was placed on the table. An incision, three inches and a half long, was made, beginning an inch and a half higher than the superior anterior spinous process of the ilium, and one inch distant from that process internally; being also four inches and a half distant from the umbilicus, extending obliquely downward, and terminating about one inch above the basis of the tumour. This incision, which was nearly in the direction of the fibres of the tendon of the external oblique muscle, divided the skin and adipose membrane, and exposed that tendon, which was next cut through, the whole length of the external incision. The internal oblique muscle now protruded at the wound, and was carefully cut through; the inferior edge of the transversalis abdominis was next divided, but not so far upward as the top of the external wound. My finger was then introduced, and the cellular texture readily yielded it a passage to the external iliac artery, the trunk of which I distinctly felt pulsating very strongly. With my finger I separated it gently from the neighbouring parts; but took care to denude only a very small portion of the vessel. The peritoneum I was equally careful to detach as little as possible; and not more than a square inch of it was disturbed. The only remaining difficulty in the operation was to pass the ligature round the vessel; and this having been anticipated, was readily surmounted. Before commencing the operation, I had secured an aneurismal needle (a blunt bodkin of silver properly bent) in a pair of curved forceps, by tying the handles of the forceps firmly together. The needle was armed with strong bobbin; and thus connected with the forceps, resembled a tenaculum, which could easily be managed outside of the wound. With one finger in the wound I found it very easy to direct the extremity of the needle, and with the forceps in my other hand, to push it through the fascia surrounding the vessel. The string connecting the handles of the forceps was now cut, and the needle was left under the vessel. The forceps being removed, the needle was drawn out, leaving the ligature round the artery. Convinced, by careful examination, that nothing but the artery was included in the ligature, and that it was, to the best of my judgment, natural in size and texture, I tied it very firmly, as high up as possible. The pulsation of the tumour instantly ceased. Three knots were made, and the ends of the ligature were left out at the external wound. No blood-vessel of magnitude was divided, and not half an ounce of blood was lost. No stitches were employed to close the wound; a strip of adhesive plaster effectually answered this purpose. A pledget of lint was applied, and the patient was put to bed, his thigh being moderately flexed upon the pelvis. He complained of extreme pain during the latter part of the operation, the whole of which occupied eleven minutes.

The patient's pulse, for several days before the operation, was 80: after the operation it was 88, and rose in the afternoon to 100. At four o'clock he was bled ten ounces. At seven he complained of extreme pain in the back and belly, and also of some pain in the limb. He was not permitted to take any sustenance except toast and water. The superficial veins of the leg and foot were filled; and the whole of the limb was covered all the evening with perspiration. Its temperature was examined repeatedly by a thermometer, and was five degrees colder than the other. It was covered with flannel and carded wool.

Tuesday, 20th. Passed a restless night, in great pain. To use his own language, in expressing his sensations, "he felt as if his loins were tearing apart." He was also troubled with pain of the bowels. Three grains of calomel and ten of rhubarb were given, but without procuring a stool. In the afternoon he was bled ten ounces, and a purgative injection was ordered; after which his bowels were freely opened, and his pain subsided. An enema, consisting of a hundred drops of laudanum, and two ounces of water, was administered, and he soon after fell asleep. The weather, on the day of the operation and several days after, was very hot. The mercury of the thermometer in the patient's room stood at 86° Fahrenheit. Placed between the toes of the aneurismal limb, it rose to 88°; between those of the sound limb 90°; at both knees it stood at 92°. His pulse was 100, and tense.

Wednesday, 21st, *third day* after the operation. The sleep procured by the anodyne injection continued all night. In the fore part of the day he was easy; but in the evening his pain returned with considerable fever. He was bled ten ounces, and took ten grains of magnesia and as much rhubarb: this, with the assistance of a clyster, brought away a large quantity of fæces and flatus, and procured relief of all his pain. The anodyne injection was again administered, and he soon after slept. His pulse 100, and somewhat tense.

Thursday, *fourth day*. He slept all night, and was much better; being quite free from pain and fever. His pulse 90. He ate some boiled rice with great relish. The wound was examined, and it was found that nearly all of it had united: a little healthy pus surrounded the ligature. The limb was four degrees colder than the sound one.

From this time no change of importance occurred until Sunday, 1st September, when the ligature came away; viz. on the fourteenth day after the operation. In a few days more the wound cicatrized, without the occurrence of a single unpleasant symptom. On the twentieth day after the operation, his nurse being absent, he arose from bed and walked across the room, and has taken exercise every day since without inconvenience. The tumour in the groin diminishes slowly, and at this time is much reduced in size.

REMARKS. The operation of tying up the external iliac artery above Poupart's ligament, was first performed by Mr. Abernethy, under circumstances in which immediate death was the only alternative. He repeated it afterwards in cases of aneurism seated so high in the femoral artery as to preclude all prospect of a cure by any other means. He performed the operation four times. In the first two instances his patients died; in the succeeding cases they recovered.* Mr. Freer, in the Birmingham hospital, performed the operation in a case of inguinal aneurism with complete success; and soon after another case was treated successfully by Mr. Tomlinson, in the same hospital.† These six cases are all I have seen related, in which the operation has been tried. The case I have now detailed is the seventh; and it has failed in only two of these. In every instance the limb has been supplied with blood, which does not uniformly happen after the operation for popliteal aneurism.

I wish, before closing this paper, to call the attention of those surgeons who may have occasion to perform the operation, to the forceps, an engraving of which is annexed. This instrument was contrived several years ago by Dr. Physick, for the purpose of passing a needle under the pudic artery, when wounded in lithotomy, and has since been used by him for securing bleeding arteries in deep narrow wounds. Mr. Abernethy complains of "the great difficulty of turning a common needle in a deep narrow wound;" and Mr. Freer was unable to pass his aneurismal needle round the iliac artery, until he punctured the fascia surrounding it

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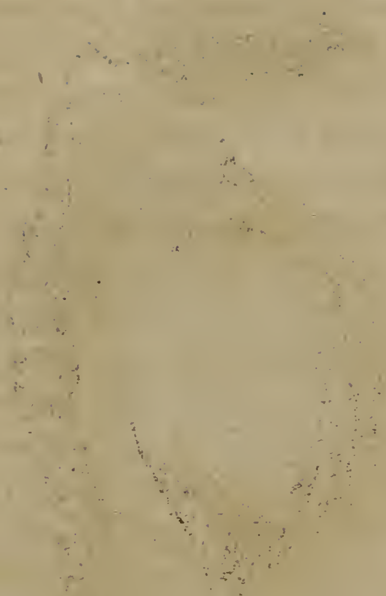
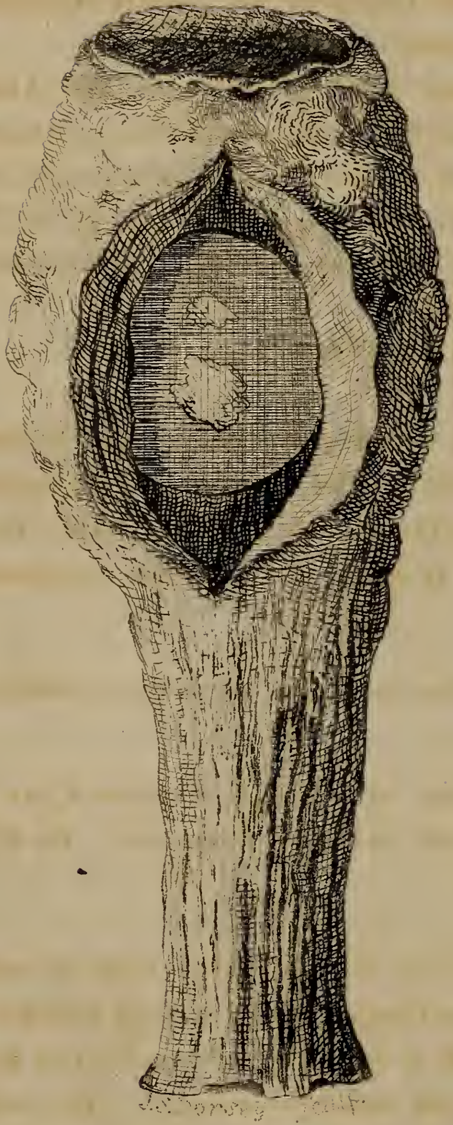
* Abernethy's Surgical Observations.

† Freer on Aneurism.





COPPER IN THE ŒSOPHAGUS.

with his knife, which he confesses was the most "difficult and dangerous part of the operation." These dangers and difficulties are entirely obviated by means of the curved forceps; and I think the operation greatly simplified by the use of this contrivance.

Should this paper meet the eyes of Mr. Abernethy, I hope he will be gratified with the additional testimony of the importance of an operation for which the world is indebted to the intrepid efforts of true genius; and he will no doubt learn with pleasure, that one individual on this side the Atlantic owes to it his life.

Arch-street, 30th September, 1811.

☞ The sketch which accompanies this communication was intended to point out the part where the operation was performed, and the instrument used to convey the ligature round the artery. It needs no particular explanation.

ACCOUNT *of a* CASE, *in which a* COPPER COIN *remained about thirteen Years in the* ŒSOPHAGUS. *By* JOHN SYNG DORSEY, *M. D.*

I WAS requested, on the 8th May last, to examine the body of a young gentleman who had been attacked the preceding evening with a vomiting of blood; ten hours after the commencement of which he expired. He had been visited by Dr. Rush, who administered, without any benefit, very considerable doses of sugar of lead and opium.

He was a native of Dublin, aged seventeen, and at four years of age, in playing with a half-penny, it unfortunately slipped into the œsophagus, and was there arrested. Some of the most eminent surgeons of that city were sent for, and endeavoured to extricate the coin, but their efforts were unavailing. His health for many years after was extremely delicate. Dr. Gregory, of Edinburgh, was consulted on his case, and predicted its fatal termination. He had always experienced great difficulty of deglutition, and was obliged to masticate his food very well before attempting to swallow it, and he often suffered so much distress at dinner as to retire from the table. He was also frequently affected with dyspnœa. He had never been attacked with hæmatemesis until the 7th of May, when he complained at about five o'clock in the evening of nausea, and immediately discharged a large quantity of blood, which continued, with short intervals for ten hours, and at three o'clock in the morning he expired.

The stomach was opened about eight hours after death, and found to contain above a quart of dark coloured fluid blood, mixed with small coagula, and with mucus. This was removed, and the villous coat of the stomach being cleansed, appeared extremely red and inflamed. I was convinced that the redness was not entirely the consequence of any stain from the contents of the stomach, because it was not universal, but in spots; and though every part of the stomach was in some measure stained by the transudation of the liquor contained in it, yet when washed in cold water it was very easy to distinguish marks of inflammation; the villous coat being thickened and elevated at the inflamed parts. I next made an opening into the œsophagus, behind the lower part of the larynx, and passed my finger without any obstacle down towards the stomach; upon introducing my finger however into the lower extremity of the œsophagus, through the cardiac orifice of the stomach, and forcing it as high upwards as possible, it came in contact with the edge of the copper. I removed the parts, and upon particular examination it appeared that the half-penny had lodged vertically in the œsophagus, immediately behind the bifurcation of the trachea; its flat surfaces presenting forward and backward—so that before and behind it there was sufficient room for a finger to pass. That part of the œsophagus which invested the coin was found considerably thickened and indurated—it had contracted adhesions posteriorly to the spine, and anteriorly to the trachea.

It was very remarkable that the coin was quite loose in the œsophagus, so that the slightest force removed it upwards from its situation; it could also have been pushed downwards into the stomach; but in this direction there was more resistance. Upon being very carefully removed and examined, it appeared coated with a hard blackish matter; in size and thickness it resembled an English guinea, or an American half-eagle; some of the letters of the inscription could be distinguished, and at one part a scale of the dark oxide was found to have been removed, and there a metalic appearance was evident; the scale detached must have been small, and probably would not have weighed two grains.

The inner coat of the œsophagus where it embraced the copper, was covered with small white projections like the matter of adhesions, some of which were a quarter of an inch in length, and pendulous.

From the preceding statement there can be no doubt, that for the period of thirteen years, the half-penny had remained in the œsophagus. In what manner it was so long detained there, is a question upon which the dissection seems to throw very little light, as it appeared to be perfectly loose and moveable.

I have preserved the parts so as to show by an incision through the œsophagus, the copper in the situation in which it was found. The preparation very much resembles one in the Hunterian Museum, in which a half-crown is exhibited in the œsophagus of a man who died shortly after an attempt to swallow it. An engraving of this may be seen in the plates of Baillie's Morbid Anatomy. It differs however in one respect; there is no alteration of structure in the œsophagus, as the patient died before this effect could take place.

An Extract of a Lecture, introductory to the opening of the College of Medicine of Maryland; delivered by J. B. DAVIDGE, *M. D. &c. &c.*

PATHOLOGY.

"THE remote causes of diseases *all* unite in producing but *one* effect, that is, *irritation* and *morbid excitement,* and of course are *incapable* of division. The proximate cause of disease is an *unit.*" *Rush's Introductory Lecture,* p. 151.

All analytical disquisitions on the modus operandi of causes producing disease in the animal body, are illusory, and perhaps will continue to be unproductive of conviction or satisfaction, at least, until the principles of life shall be more fully ascertained. Causes probably act upon the living, the nervous system, not unlike external objects do, through the intermediation of the senses, on the intellectual powers. The latter, by acting on our senses, give *occasion* to perception, to thought, and lead on to induction or judgment; the former act on the living system, and give *occasion* to those primary changes, accompanied by disturbed sensation, or inordinate action in, or interruption of the function of an organ or organs, which we style disease.

This original or primary change in, or departure from the healthy condition of the body, must for ever be in kind according to the nature of the operating cause; and equally with the causes susceptible of division. Thus, that disease which we term small-pox; or that which we term intermittent fever; or that which we term lues venerea, is referable for its peculiar phenomena, to the nature of the particular cause acting on the animal system. The virus of the small-pox will never, under any circumstance whatever, produce the phenomena of the lues venerea; nor vice versa.

That every disease is an *irritation* or *morbid excitement*, is a position, so plain and so true, that it neither admits of refutation nor illustration. It is one of those self-evident propositions which defies argument, and is unsusceptible of proof; it is an obvious medical axiom. But that every *irritation* is the *same in kind*, and of course incapable of division, needs some proof; more we apprehend than has as yet been laid before the public.

The immediate and necessary consequences of the assumption of the learned Professor, that morbid excitement is simple and *incapable of division*, and that the proximate cause of disease is an *unit*, are that all the morbid appearances or pathological changes, which present themselves to our view, can be nothing more than varied forms or modifications of the same generic disease.

It would be equally defensible and logical, and by no means remote in analogy, to argue, that because the principle of animation or state of being enlivened, or the principle of vegetation or power of producing plants, is simple and incapable of division, therefore, all animal or vegetable productions, are nothing more than varied forms or modifications of one genus of animals or vegetables. If we are permitted to indulge in that species of abstract induction or metaphysical sophistry, we should analogically arrive at the conclusions in zoology or botany, that the Professor would in pathology.

What zoologists would, upon the fact that the horse and cow are sustained in life by the same principle of animation, deductively arrive at the conclusion that these two animals are only varied forms or modifications of the same genus?

And yet, upon the strictest analogy, he would only follow the rule of philosophising laid down by Dr. Rush. He states disease to be an *irritation* or *morbid excitement,* and immediately proceeds to his conclusion, that, of consequence, every disease must be the same, and all morbid affections necessarily incapable of division.

The zoologist, arguing from the difference of external figure and internal structure, determines the horse and cow to be of distinct genera or kinds. And, in our opinion, there is no exception to be taken to the ground of his reasoning, or to his conclusion. The botanist, proceeding on similar principles, has no difficulty in concluding the oak and the pine to be distinct in kind. And we expect to prove that one disease may be, and really is generically different from another, although the learned gentleman has, according to our opinion, rather boldly asserted, that the remote causes of diseases *all* unite in producing but *one* effect.

It is generally granted, and we believe there is no insecurity in the conclusion, that in the animal or vegetable kingdoms, each genus, which, under no temperament of constitution, or operation of climate, can be produced by any other, is distinct and separate from every other genus. And, with an equal parity of reason, *that* disease, which, under no variety of constitution or dissimilitude of climate, can be produced by any other, may, in common sense and sound philosophy, be determined to be distinct from all other diseases. This is so plain, that we apprehend it may stand as an axiom, against which there can be no reasonable objection, and on which we will put at issue our discussion.

The intermittent or remittent fever, varieties of disease from the same remote cause, and of the same genus, may attack the human body at any period of life, and may be removed by the unaided efforts of the constitution, or by these efforts artificially aided; or they may, from their inherent violence, or the inability of the body to resist, prove fatal. This genus of disease can make its attacks once, or oftener through life; it leaves no taint in the parent body, transmissive either to near or remote offspring: it is derived, so far as ætiological researches afford evidence, from an insalubrious atmosphere, and is not contagious. We do not at present speak of the distinctive symptoms of this genus of disease; a consideration of them being postponed until we shall arrive at a more advanced stage of our discourse. We are now disposed to limit ourselves to the obvious facts of its operations on the human system.

The gout seldom attacks until the body has arrived at its acme or majority; does not appear to be owing, in its remote cause, to any insalubrious condition of the atmosphere, as it takes place under every variety of healthful and unhealthful modification of the air. Having once subdued the animal economy to its laws, no efforts of the constitution, or endeavours of art, can ever after, through life, relieve the body from its influence. Nor is its influence limited to the parent body; it descends to the child, and may continue to inquinate the body of the descendants for generations.

There are not, between the horse and cow, any circumstances which can more certainly mark them to be of separate genera, than there are between the remittent fever and gout, to designate them as generically distinct diseases. But we proceed.

The small-pox, inscrutable in its origin, assails the human body at any, and every stage of life; runs its course in fifteen or twenty days; can be propagated by effluvia, from the diseased to the healthy; leaves no taint that can be transmitted from parent to child; nor does it but once attack the same body. Laying aside the particular sensible expressions of the gout and the small-pox, while operating on the constitution, they appear to our understanding, to be as specifically distinct from each other, as the sheep and the hog, or the oak and the pine,

From the insertion of the variolous virus into a body that has not antecedently passed under its action, whether the body at the time of the insertion be healthful, or labour under a remittent fever, or gout, a genuine legitimate small-pox will be produced, and capable of reproducing itself. Or if the virus of the small-pox be inserted into a wound, made in a person under the measles, it will take effect locally, but will not evolve itself and come fully into operation throughout the whole system, until the action of the measles shall have spent itself; then will the small-pox virus unfold its nature and full character, *unmixed* and *unaltered* by any combination with the measles.

Here the germina of the measles and small-pox come as much as possible into union; yet the variolous virus obtained from a pustule in a person who had just undergone the morbillous action will produce the genuine small-pox.

If the ass-genus copulate on the horse, the production will be a mule, or hybrid animal, and unfertile. This obtains also with other animals; hence it appears, without any strained induction, that diseases, under certain circumstances, may be more *uniform* and *fixed* in their laws, and blend less their characters than some animals or vegetables.

The lues venerea attacks at any period of life; but, unlike the small-pox and measles, and several other diseases, it does not run its course in a limited time; leaving the body unsusceptible of any future return. If it be not opposed by art, the patient's life will inevitably be lost, and that with an unerring uniformity.

Will gentlemen who have made up their minds in full accordance with all the consequences of the new philosophy of simplicity and unity, argue that there is no essential radical difference between the *irritation* of the gout, and the *morbid excitement* of the small-pox; between the *irritation* of the vaccine disease and the *morbid excitement* of the lues venerea? Will they insist that "their remote causes *all* unite in producing but *one* effect;" or that the proximate cause of the gout, the small-pox, the vaccine, and the lues venerea is but an *unit* and the same?

This putative unity of disease reminds us very strongly of the ancient alchymy, which maintained that there is but one true metal; that all metallic bodies were only so many modifications of this one genuine metal; and that they were all reducible by chemical process to this one true metal, gold. The analogy between the present medical philosophy and that of the alchymists is not remote, and we apprehend when the new doctrines of pathology shall be examined with a like degree of minuteness and care, as were the alchymical doctrines, they will share a similar fate. Their premises were, without doubt, as well laid, and their inductive reasoning as good, and equally plausible.

"Pathology has for its objects, the remote, exciting, and proximate causes of diseases." *Rush's Introductory Lecture,* p. 14.

Pathology has no more to do with the remote, exciting, or proximate cause, than the doctrine of plants has. Such a construction of the term is not to be justified either by etimology or general use. All inquiry into remote and exciting causes, falls altogether within the range of ætiology, and belongs to it alone.

Innovations in the technical language of science, particularly when such changes tend to corrupt the language, are followed, and that of necessity, by confusion of the most serious kind, not of words, but of ideas. It is equally necessary that the terms of a science should be defined, as that its principles should be fixed. Our *ideas* can only be made public, or communicated, by *words* or signs, and if these be loose and undefined, there is an end to all exchange of sentiment and reasoning.

Pathology is the philosophy of the manner in which one morbid change in the human body, succeeds to, and is produced by another. It treats of the manner in which causes affect the animal economy, but cannot extend to a disquisition on the nature or origin of causes. Pathology explains, for instance, how and from what series of causation, pain, increased heat, redness, tumefaction, arise in an inflamed part; and also the pulsation of the neighbouring arterial trunks. If we travel into the nature or origin of the causes, either remote or exciting, we unquestionably forsake the limits of pathology, and get within the province of ætiology.

"Nosology presupposes the characters of diseases to be as fixed as the characters of animals and plants; but this is far from being the case. Animals and plants are exactly the same in all their properties, that they were six thousand years ago; but who can say the same thing of any one disease? They are *all* changed by time, and still more by climate, and a great variety of accidental circumstances." *Rush.*

Nosology has for its object the nature and pathognomonic symptoms of disease. It is simply the science of pathognomonics, or that series of diagnostic symptoms, which is inseparable from, and uniformly indicative of a disease, and by which that disease is to be known from every other. And we are prepared to *say,* and entertain hopes of sustaining our assertion, that the pathognomonic symptoms in diseases, are as unequivocal and fixed, as the distinctive characters in animals or plants; and further, that no generic disease, in any of its distinctive diagnostic properties, has ever, by time, by climate, or any other accidental circumstances, been changed.

The better to avoid misapprehension, unfairness, or illusion, we take our characters from Cullen's Nosology; yet we are not to be understood as taking upon ourselves the defence of all his opinions and peculiarities.

The diagnostic series of symptoms of the intermittent fever, or that by which it is distinguished, is

"Febres, miasmate paludum ortæ, paroxysmis pluribus, apyrexia, saltem remissione evidente interposita, cum exacerbatione notabili, et plerumque cum horrore redeuntibus, constantes: paroxysmo quovis die unico tantum." *Cullen.*

Pneumonia.

"Pyrexia, dolor in quadam thoracis parte; respiratio difficilis; tussis." *Cullen.*

Podagra.

"Morbus hereditarius, oriens sine causa externa evidente, sed præeunte plerumque ventriculi affectione insolita; pyrexia; dolor ad articulum, et plerumque pedis pollici, certe pedum et manuum juncturis, potissimum infestus; per intervalla revertens, et sæpe cum ventriculi, vel aliarum internarum partium, affectionibus alternans." *Cullen.*

Variola.

"Synocha contagiosa cum vomitu, et, ex epigastrio presso, dolore. Tertio die incipit, et quinto finitur eruptio papularum phlegmonodearum, quæ, spatio octo dierum, in suppurationem, et in crustas demum abeunt, sæpe cicatrices depressas, sive foveolas in cute, relinquentes." *Cullen.*

Now permit us to appeal to professional men of reading, of observation, of candour, to determine whether there be in any written authority, ancient or modern, or in their own personal experience, any series of facts, from which they could inductively conclude, that the above diseases, or either of them, have been radically or fundamentally changed; that any diagnostic has been added to, or taken from those diseases by *time*, or by *climate*, or by *accidental circumstances.*

We do not mean that all persons shall have all the symptoms with uniform precision. It would as rationally be expected that the horns and colour of every cow would afford an uniform sameness.

But it may be suggested, that, not unfrequently, inflammation and the intermittent unite in the same habit at the same time. Of this interesting fact we are not unapprised. But by what means do we ascertain this union? Is it not by the nosological characters alone that the practitioner regulates his measures? by the nosological signs that he recognizes the very existence of the thing? He does not surely know it by intuition. No inflammation can possibly arise in any part of the body without showing the nosological characters proper to it, viz. redness, increase of heat, tumefaction, and pain. And if it be in any of the important organs, it is discoverable by the appropriate signs of an inflammation of that organ, and by these signs only.

But with all the heterogeneousness of character, and blending of symptoms in disease, nosology can never be under greater difficulties, or in greater uncertainties, than zoology and botany in their mules and hybrids.

Upon no other ground could Sydenham bottom the opinion that the history given by the Arabians of the small-pox was superior to any other, or that it was at all correct, than on the exact correspondence between that disease as described by the Arabians, and as it appeared under his own observation in London. If "*time, and still more climate,*" could effect changes in this disease, it should have suffered some alteration in the long tract of ages which have elapsed since the days of Rhazes and Avicenna; seeing it has not been kept to one region only, but has been diffused through all nations, and spread over all countries.

Has *time,* too, laid its hand on the lues venerea; or has *climate* written on it those changes to which, according to Dr. Rush, all diseases must pay homage?

The vaccine disease is, at this moment, rapidly diffusing itself through every region of the civilized world; and, as far as we are authorized to form our conclusions, retains all its distinctive characters when in its genuine state.

Professor Rush, we find, does not limit his pen to one or two diseases, if indeed one or two could be found to give him countenance; but he unhesitatingly avers, that "they are *all* changed by time, and still more by climate, and a great variety of accidental circumstances."

Of this general and sweeping proposition we should have a much more respectable opinion, were it corroborated by any specification of facts; or had any one generic disease been pointed out which had been obviously and acknowledgedly changed by either of the circumstances referred to. In short, upon what expectations or hopes is it that a Professor teaches medicine, if an inflammation, an apoplexy, a yellow fever, or vaccine disease be not marked the following, by the same symptoms it was the preceding year; or if it be not recognized under the same form in North as in South-America? What is taught in Philadelphia cannot be true in either the eastern or western extremity of our country, if there be this constant flux and reflux in the nosological characters of disease.

If the yellow fever does not appear under the same general form as is described by Rush himself; or the apoplexy, or measles, or pleurisy, as described by Cullen, in every part of the world where the disease itself appears, we should be gratified in knowing by what badges it may be discovered, or how recognized. We do insist, that if diseases be so mutable in their diagnostics, medicine cannot be a science; it is a mere art, and a very crude one too, of conjecture. Nothing short of long experience and an examination into the seats of diseases, can either ascertain the morbid conditions of the body, or the signs of these conditions, by which alone they can be judged of. Every new disease must be accompanied by new and unknown symptoms, and of consequence the physician's learning can be of no possible service. Nay, the experience of every year becomes useless, as with the new year flows in a new tide of diseases, and he has to commence anew.

"But the morbid state of the system often assumes in the course of a few days *all* the symptoms of a dozen different genera of diseases. Thus a malignant fever frequently invades every part of the body, and is at once or in succession an epitome of the *whole* class of pyrexia in Dr. Cullen's Synopsis." *Rush's Introductory Lecture.*

The whole of this paragraph is written with that seriousness and gravity which is well calculated to secure the easy confidence of the careless reader; and he would feel himself persuaded that it is at least probable, if not exactly true. But the whole is altogether erroneous, and indefensible throughout. Has the morbid state of the system ever been such as to assume the symptoms of the chicken-pox, measles, and small-pox, much less a dozen different genera, in the course of a few days? What malignant fever is it, that is at once, or in succession, an epitome of the whole class of Dr. Cullen's pyrexia? Is it the yellow fever, or plague, that is at once, or in succession, an intermittent, a pleurisy, a chicken-pox, a measles, a whooping-cough? &c.

We thought it had been conceded, long since, by common observation, and common sense, that no two general diseases of the class of pyrexia could be present in the body at the same moment. Indeed, we had admitted the belief that it would be equally rational to maintain that two atoms of matter could occupy the same space at the same instant, or that the human mind could contemplate the past and the future simultaneously, as that any two of those general diseases alluded to could co-exist. Or is it meant that the symptoms of a dozen different genera of diseases could be present, and yet the diseases themselves not be in operation?

But we are informed also, that "a malignant fever frequently pervades every part of the body." Fever, every fever we suppose, is a convulsive action of the arterial system; every part therefore provided with arteries, and no living part can be without them, must be affected, or, if the writer pleases, pervaded by not only every malignant, but every other fever. A pen of far inferior note might have been employed in promulging this well known fact without being entitled to much credit.

"Nosology has led physicians to prescribe exclusively for the names of diseases, without a due regard to the condition of the system." *Rush's Introductory Lecture,* p. 153.

There must be some misconception in the circumstances of this objection. That physicians could have been led by nosology to prescribe exclusively for the names of diseases, without a due regard to the state of the system, is what we do not believe. It is too absurd. There are but two grounds upon which a physician can prescribe for a patient. The one is, he receives a report either directly or indirectly from the patient, purporting that he labours under some specific disease, a pleurisy, a small-pox, or syphilis, and on that report he prescribes.

In this case, it cannot ingenuously be said, that he prescribes either exclusively or at all for the name of the disease. He most assuredly prescribes for the *condition* of the system of which the name is significant, supposing the reporter has considered the symptoms, and inferred the condition, to which he has given a specific name.

The other, is that where the physician takes the distinctive characters immediately from the patient himself, and, arguing from effects to causes, concludes synthetically on the condition of the system, to which he may give an appropriate epithet, and prescribes accordingly. In neither of those instances can it ever be surmised, that the physician prescribes exclusively for the name of the disease, or that he has any regard to it.

In the first case, he prescribes imprudently and at random, because he permits an unqualified person to judge for him. The name, in both cases, is a mere incidental thing, and added solely on account of the facility of communicating the fact, as it relates to the morbid state of the body. The reporter, whether the sufferer himself, or a friend, when he communicates the name of the disease, intends to convey also the condition of the system; when he gives the sign, he also communicates the thing signified.

" Nosology unnecessarily multiplies the articles of the materia medica, by employing as many medicines as there are *forms* of diseases." *Rush's Introductory Lecture,* p. 153.

In this charge is conveyed a reproach against nosology for instructing physicians to prescribe for the *forms* of diseases, and that nosology thereby unnecessarily multiplies the articles of the materia medica.

The patient himself has no knowledge of being diseased, but through his feelings, and those feelings he communicates to the physician. What are the feelings communicated to the physician, but assemblages of distinctive sensible symptoms, or *forms* of diseases? And if the physician is not to prescribe from a consideration of those assemblages of symptoms, or *forms* of diseases, arguing from them the nature of the disease, we should be gratified in being informed upon what rational ground he is to prescribe at all. We believe but few physicians affect to ascertain intuitively the conditions of the symptoms of their patients. To prescribe for a disease from a due consideration of its *form,* or distinctive marks, we conceive to be one of the soundest lessons of nosology, and one of the wisest dictates of an enlightened medical education. But how does this multiply necessarily or unnecessarily, the articles of the materia medica?

The science of disease is a grand whole, and like every other science, is made up of parts. The first act of nosology, is to separate these parts, and arrange them into order and system, according to their approximations in character. From this we gain important advantages; our minds are not distracted or overwhelmed by a confused multitude of heterogeneous, incoherent, mutually repulsive materials. But we have the advantage of entering on each part separately, and, when its intricacies and contexture shall have been examined, we engage in a second, under the same auspicious circumstances. Thus are the facilities to apprehension and memory, exceedingly multiplied and increased. It is by piling one examined fact on another, that the fine edifice of a complete professional education is to be raised.

By this particularity of knowledge, we acquire a dexterity and skill in tracing out morbid conditions from pathognomonic signs, not only more readily, but much more certainly than we could, were we to attempt the whole in chaotic mass. To attempt the whole, without order or rule, is like a learner wasting his time, and exhausting his powers, in efforts to read, before he has acquired his alphabet, or mastered the elementary constituents of language: or indeed, like a sailor, cast out to sea, without compass or quadrant; he may possibly get into harbour, but more probably his ship will be wrecked.

The following objection to nosology, stands first in Professor Rush's list, but we have postponed it to the last, in order that it may make the better impression on the reader's mind; it being in our estimation, the most singular specimen of logic that has ever come under our notice.

" It (nosology) precludes all the advantages which are to be derived from attacking diseases, in their *forming state*, at which time they are *devoid* of their *nosological characters*, and are most easily and certainly prevented or cured." *Rush's Introductory Lecture*, p. 153.

That a disease can exist in its "*forming state*," or any of its stages, without the nosological characters appropriate to that state or stage, we can as readily conceive, but not more so, as that matter can exist without properties of extension, figure, or divisibility; or that mind can be present in the body, without its attributes of perception, thought, or memory, and that we can have knowledge of the existence of matter, or of mind, thus circumstanced.

If there be no *nosological characters*, or diagnostic symptoms, during the state of diseases; that is, no disturbed sensations of which the patient is conscious or sensible, or which are palpable to the senses of the physician, by what means does the physician know that there is a forming state of diseases? It appears from the express words of the professor, that the physician is not only to know diseases to be forming, but is also to prevent or cure them. The physician is successfully to interfere with the forming state of diseases, when there is no character, no evidence, no symptom of disease! For the Professor says, "*they are devoid of their nosological characters* in *their forming state!*"

OBSERVATIONS *on* PERUVIAN BARK. *By* CLARK SANFORD, *M. D. of Greenwich, Connecticut.*

THE importance of making any improvement in the preparation of so valuable a medicine as the Peruvian bark is obviously so great, that if any hints herein stated should be deemed to promote that effect, I shall not doubt but that you will receive this indulgently. No article of the materia medica has probably undergone so many and varied experiments for this purpose, as well as for ascertaining its virtues, as the bark.

In the early periods of its use the ill consequences that followed its misapplication, or indiscriminate use in every malady that bore the slightest anology to those which had yielded to its influence, procured it many enemies; while latterly, the inferior quality or condition of the article itself, has had no less weight in producing a very considerable luke-warmness to its character.

The prejudices arising from the first cause were dissipated in proportion as the improvements in pathology advanced. Since many diseases, exhibiting apparently the same weakness and general phenomena, are now well known to proceed from two opposite and contrary conditions of the system, the one requiring increased, the other diminished tone and action; hence directing the physician's attention to the existing error instead of the name of the disease; it seems probable that we have arrived nearer to certainty in the application of this, as well as we have done in the use of the lancet, and many other material remedies. But the indifference depending on the second cause has still much reasonable foundation. As the inferior quality and bad condition of this article are predominating with increasing effect, and seem likely to diminish, if not destroy its importance to the healing art, I think it cannot be deemed useless to investigate the causes of this depreciation.

Before the immense importations of inferior and spurious kinds of bark with which our markets abound (and which the relaxed state of Spanish authorities in South-America have favoured) we had but little cause to complain of their quality; but in the present state of this article, it is no less necessary to discriminate in the choice than it is in its administration.

Until about the year 1790 there were only two species of bark in use, to wit, the pale and red. Soon after that period, a third species, the yellow, was introduced; and at this time there are nearly a dozen different varieties, principally of the yellow kind, selling under the boasted title of Cinchona. About the year 1790, the exorbitant price of red bark encouraged a spirit of adventure in search of that article, which brought into this country a superabundance, a considerable proportion of which, although agreeing in colour with the genuine, was found to possess but little medical power. The same causes have more recently produced a similar result in procuring the introduction of many species and varieties differing greatly in colour and quality.

The great difference in the size and thickness of the pieces, in texture, strength and sensible qualities, as well as in colour, afford the most conclusive evidence, that many different trees, bearing a greater or less degree of affinity to each other, furnish the different barks of commerce at this day.

To point out the uncertainty of relying on the bark as it is now generally met with; to state some experiments made on different kinds and preparations deemed of most practical use, without detailing those minuter ones which have been so often repeated by abler experimenters, and with such different results; to point out rules for selecting; the best mode of pulverizing and otherwise preparing the bark for exhibition; and to invite your attention to some preparations of my own, is the object of the present address.

It is important in selecting bark to know, that its quality depends more on its texture than on its colour.

Every piece of Peruvian bark, of whatever colour, texture, or quality, exhibits three laminæ, viz. the exterior or epidermis, the central or parenchyma, and the interior or semiligneous. These appearances are more distinctly observable in the yellow than in the other species of bark, particularly in the inferior varieties, in which one half or more of the inward surface appears to be composed of strong parallel fibres, of a woody nature, difficultly reduced to a fine powder, and possessing but little sensible properties. The great or small proportion of the parenchyma, if sound, compared with the woody portion, serves as the best criterion for choosing bark in the crude state. In the best parcels, the parenchyma prevails nearly throughout the whole thickness of the pieces; this breaks short and brittle, and appears dense. These appearances generally attend both the pale and red, but with some exceptions.

The epidermis of the pale and red is so thin and so interwoven with their corrugated surface, that no attempts have been made to separate it, the whole mass being ground promiscuously together. But the epidermis (which is an inert and useless substance) being thicker on the yellow, and adhering but loosely to its smoother surface, is principally separated from it before it is exported from South-America, leaving the parenchyma encumbered only with the woody portion.

A particular examination of the following species and varieties, which constitute nearly all the different kinds now employed under the name of Peruvian bark, will serve to throw some light on the general nature and comparative value of each. For the sake of reference merely, I have thrown them into numerical order.

No. 1. Superior red. This comes in large thick pieces, has a thin epidermis, which adheres firmly to its rugged surface; is heavy, hard, and brittle, and breaks short and shining. The parenchyma is thick, compact, and plainly distinguishable from the inner semiligneous portion; taste strongly bitter, and astringent; smell somewhat peculiar, although common to bark; yields, under a particular mode and given degree of grinding, a large proportion of impalpable powder, though less than the pale.

No. 2. Inferior red. Light and spongy; comes with little or no epidermis; parenchyma and semiligneous portion not easily distinguished. This, from the deficiency of its sensible properties and its exceedingly tender structure, appears to be a decayed bark. The whole substance is reduced more easily than any other to a fine powder. This and the preceding, owing to their scarcity and the high price they both have borne, are now rarely met with in their genuine state.

No. 3. A firm bark; heavier than any of the others; general thickness from one to three lines; external surface hard, smooth, and principally without epidermis; parenchyma dense and brittle, and constituting nearly the whole substance; semiligneous portion scarcely visible, and breaks with little or no shivery appearance; taste intensely bitter and considerably astringent; colour in powder a yellowish red, approaching more nearly than any other to the true red bark; smell brisk and common to bark; yields more impalpable powder than any other, except the decayed red last mentioned. This appears to be a strong and valuable bark, lately introduced; but owing to its high price and better demand in Europe, but little has been retained here, and is probably not to be found pulverized in the shops of this country.

No. 4. Common pale bark; has a thin grey-coloured epidermis, which is interwoven with its uneven surface; is mostly rolled up in short thin pieces. In texture it nearly resembles the superior red. The fracture is short, smooth, and shining; parenchyma thick, and scarcely distinguishable from the semiligneous portion; taste bitter and astringent; smell common to bark, but stronger than in the others. Although a hard bark, it affords a large proportion of impalpable powder.

Another kind of the same colour has been noticed, differing but little from the other, except in the size of the pieces, which are larger, thicker and wider.

No. 5. First quality of yellow; in large and small flat pieces, from one half to two lines in thickness: surface slightly corrugated; epidermis very thin and easily separated; parenchyma extending nearly through the whole substance; breaks short and brittle, but not shining, with but little fibrous appearance on its inner surface; colour a rich lively yellow; taste strongly bitter, with slight astringency; smell common to bark in general, with the addition of a slight cinnamon flavour, especially when heated; is capable of yielding a large proportion of impalpable powder.

No. 6. Second quality of yellow. The pieces are thicker and longer than the last mentioned, but not so broad, except a few pieces which appear to come from the tree near its root; thickness from one to three lines; surface shrivelled; epidermis thin and adhering closely; parenchyma thick and scarcely distinguishable from the more ligneous inner surface; breaks short, but not so brittle and shining as the pale; yields like the former, a large proportion of impalpable powder; taste bitter with slight astringency; smell brisk and common to bark, without aroma; colour a pale yellow, approaching to the colour of the pale bark.

No. 7. Third quality of yellow; generally rolled up in the quilled form in larger and longer pieces than the pale; surface somewhat smooth; epidermis very thin, light coloured, and adhering firmly; parenchyma thin, hard and brittle, and easily distinguished from the semiligneous portion, which is thick, tough, and shivery; thickness from one to two lines; taste pure, but not strong either in bitterness or astringency; colour a yellowish ash; smell common to bark; yields but a small proportion of impalpable powder, the remainder being very fibrous.

No. 8. Fourth quality of yellow. Of this general description, several varieties have been examined, all of which agree so nearly in texture, in the proportion of their component parts and in their sensible properties, as not to require a separate description. General thickness from two to ten lines; epidermis thick, soft, and adhering loosely, with signs of decay; parenchyma about one third of the whole substance, tender and spongy, and readily distinguished from the semiligneous portion, which is light, very fibrous and shivery; colour in powder a reddish yellow; taste feebly bitter, and very slightly astringent, and approaching to the taste of rotten wood; smell weak, with a mixture of the flavour common to bark and the faint smell of rotten wood; yields very readily a small proportion of impalpable powder, while the rest remains shivery, lighter coloured, woody and fibrous.

Another kind of bark deserves to be noticed, more on account of its similarity in appearance when powdered, to some of the others, than on account of any real resemblance in medical virtues, and because large quantities have been noticed in market. It comes in pieces of very uniform length and breadth, which appear to have been sliced from the tree while in sap; thickness in general from one to three lines, but in some pieces as much as six. In general appearance while in quill, it has a nearer resemblance to the bark of the birch tree than to any other; colour in powder a reddish ash, inclining to yellow; external surface hard, and in the larger pieces full of transverse fissures; smell not common to bark; taste scarcely bitter, but powerfully astringent. Different from any kind of Peruvian bark, it gives out to cold water a brownish red colour. It will not cure an intermittent. A cold infusion of it differs from a similar infusion of real Peruvian bark, in remaining, like oak bark, much longer free from putrefaction, in producing a ready and copious precipitate with a solution of gelatin, in forming no precipitate whatever with infusion of galls, as the latter does, and in striking with chalybeates a much darker colour. When treated with water and alkohol, it affords nearly twice as much extract as Peruvian bark; which, however, is by no means to be considered as an evidence of superior strength as a medicine, since oak bark, galls, cinnamon, bark of guiacum, sassafras, and many others afford a larger extract than Peruvian bark, yet no one ascribes to any of them a tonic power equal to the bark. It is only when the constituent principles of several barks are the same, that the quantity of matter extracted from each, by any given menstrua, will serve as a criterion of their relative strength.

The powdered state of all the barks being more favourable than the crude or unground, for judging them by their taste, smell or colour, they are to be understood, when their sensible qualities are here spoken of, as being examined in that state; and the proportion of impalpable powder which is here stated to be the product of particular described barks, is to be understood as meaning the proportion to be obtained under a given degree, and according to a particular mode of grinding and sifting, as is hereafter pointed out for obtaining in a degree, the resinous or parenchymatous separate from the shivery or semiligneous portion.

To obtain concentrated preparations of the bark has long been considered a desideratum in pharmacy of the first magnitude; but none of its preparations hitherto made have been found equal in medicinal power to the powder, and no solvent so capable of acting on all its component principles, as the juices of the primæ viæ.

The middle layer or the parenchymatous portion of the bark is that part in which the active principles chiefly abound. To separate this from the more bulky woody matter with which it is blended, has incited the ingenuity of many skilful experimenters; but owing to the wrong application of pulverizing machines, little progress has hitherto been made towards the completion of this object.

The component parts of many bodies, though intimately blended when endowed with different degrees of friability, are capable of being separated from each other by mechanical means.

Different opinions have been held as to that portion of bark which soonest yields in pulverizing. While some contend that the resin or parenchyma is soonest reduced, others are of opinion, that on account of its apparent hardness, it is the last which becomes a powder. It is difficult to reconcile opinions so contrary to each other, when they are sanctioned by such names as Lewis, Duncan, Coxe, Woodville, and others of great authority, without supposing that either very different modes of pulverizing were adopted, or that barks differing greatly in texture, were employed by each.

Resinous, hard and solid bark, when triturated with the pestle, adheres more or less to it and the mortar in which it is ground, and the longer the friction is continued in this way, and the smaller the quantity employed, the greater will be the agglutination of the best part of the substance. In this case, a certain portion of the resin gets into hard and solid masses, while the ligneous portion seems soonest to be in the powdered state. But if bark of the same description be placed beneath the operation of two vertical stones of moderate weight turning within a small circle, and in sufficient quantity to preserve a separation of the rolling stones from the plane beneath, of one inch or more, until the operation is completed, a contrary result will follow. In this case the more resinous portion does not agglutinate by coming into close contact with the levigating bodies, and in consequence of its greater degree of friability, is sooner than the other reduced to a subtil powder, while the remainder, still preserving the fibrous state, is more slowly reduced. To favour the separation of the fine, more spherical figured particles which are the product of the pulverized parenchyma, it greatly promotes the design to have the sieve preserved in a horizontal position, and to have the necessary agitation performed by motions in that direction. By these means most of the oblong fibrous particles, however minutely divided, by lying across the fine meshes of the sieve, are prevented from passing. When, therefore, the object is to obtain from the general mass of bark employed, a certain portion of its substance separate from the other, in a stronger and finer state than the remainder, the mode of both grinding and sifting last described is best adapted to promote the design. The superfine powder thus separated is observed to be of a much higher colour than the residue. This by further grinding may be reduced to an inferior powder, or may be advantageously used for tinctures, infusions, or for the preparation of the essential salt hereafter mentioned.

On the contrary, when the object is to obtain an uniform powder of the bark, consisting of all its component parts, the heaviest weights are to be employed, and, to accelerate the grinding, are made to operate on small quantities at once, until it is uniformly crushed and sufficiently broken to pass chiefly through the bolt. The bolt for better despatch is to be constructed and acted on in the same manner with those employed in factories of grain.

Thus, the different modes for obtaining different qualities of powder from bark of a given kind, are made to consist in the different degrees of weight and force employed, and in the manner of their application, whether on large or small quantities at once, whether by simple trituration, or by the combined action of pressure and trituration; while the different modes of separation are made to consist, not only in the fineness of the sieves, but in the manner of applying the pulverized mass to the sifting machine, whether this be done on a horizontal plane, with agitation in that direction, or whether it be done by means of the customary bolting cloth, with circular motions and perpendicular agitations.

The term resinous portion is here used in the broad sense in which it has been used by most writers, to mean that middle, brittle, or parenchymatous portion which contains the largest proportion of its active parts.

All the descriptions of true bark herein noticed, have been repeatedly administered in the course of many years practice, with a particular reference to the comparative strength of each, as well as to the comparative value of the powder obtained in different ways; from which I am enabled to say (as far as the varying circumstances of the different cases in which they were used would allow) that the efficacy of those which have been analyzed, corresponds with the quantity of concrete obtained from each, as exhibited in the following table.

Of No. 1, 2 and 7, it is regretted that the samples on hand at the time the others were submitted to experiment, were not sufficiently large to test them likewise by means of the concrete they might afford; but their comparative value has hitherto been determined by their efficacy in actual practice, and by their sensible properties.

No. 1 is high in its sensible qualities and powerful in its operation.

No. 2 is inferior in sensible qualities and feeble in medical virtues.

No. 7 is not high in sensible qualities, and on account of its tough and fibrous texture is rarely made fine enough to be valuable in powder.

In each of the following experiments one ounce avoirdupois of bark was employed. The quantity of water poured on in each infusion was one pint. The mixture was stirred occasionally for three hours, and then allowed to settle twenty-one more. The decanted liquor was passed through filtering paper, and the evaporation immediately commenced in shallow earthen plates set either in the sun or in an open oven heated to a temperature below 150° of Fahrenheit, and gradually reduced towards the close of the evaporation, until the product assumed a shining vitreous appearance, somewhat similar to dark coloured gum arabic. In the experiments with proof spirit, half a pint was poured on the residuum left after three watery infusions; the mixture was agitated daily for eight days, and decanted on the tenth. The menstruums employed were of the temperature of the atmosphere. Many trials were made with tepid and hot infusions, but with little increase in the quantity of the product. Decoctions increased the quantity, but injured the quality. The addition of vegetable alkali, magnesia, lime and ammonia to water in different proportions did not appear to increase its solvent power, but changed the taste of the product to a nauseous bitter.

	Quantity of concrete extracted by water in grains.				Concrete extracted by proof spirit after water in grains.
	1st. inf.	2d. inf.	3d. inf.	total.	
Superfine powder obtained from No. 3,	45	18	8	71	
Proof spirit on the residuum after the 3 watery infusions,					25
Remainder after the above superfine had been sifted out,	38	15	3	56	
Proof spirit on the residuum after 3 watery infusions,					20
Superfine powder obtained from No. 4,	35	15	1	51	
Proof spirit on the residuum,					21
Remainder after the above superfine had been sifted out,	30	12	2	44	
Proof spirit on the residuum,					17
Superfine powder obtained from No. 5,	43	18	7	68	
Proof spirit on the residuum,					20
Remainder after the above superfine had been sifted out,	33	14	2	49	
Proof spirit on the residuum,					13
Superfine obtained from No. 6,	40	13	2	55	
Proof spirit on the residuum,					17
Remainder after the above superfine had been sifted out,	28	9	3	40	
Proof spirit on the residuum,					13
Superfine obtained from No. 8,	14	6	1	21	
Proof spirit on the residuum,					14
Remainder after the above superfine was sifted out,	9	3	2	14	
Proof spirit on the residuum,					9

The substance obtained from bark, when treated with water as above, has acquired in Europe the name of Essential Salt. How far the addition of the spirituous concrete obtained after water, may add to the value of the salt, and whether its value will compensate for the expense of preparing it, remains to be determined by further experiments.

This preparation I believe has not as yet entered much into the prescriptions of the physicians of this country, or been the subject of as much attention as it seems to merit.

Dr. Reece, in the fourth London edition of his work entitled "Medical Guide," has noticed the salt in terms of high commendation. He says it has been extensively employed by himself and many of the principal physicians in Great-Britain; and in cases where the powder cannot be used, it is considered an article of inestimable value. Ten grains are stated by him to be equal to one drachm of the best powder.

"The tincture of the bark, on account of the quantity of spirits necessarily employed, is in many cases inadmissible; while a dose of the infusion, adequate to ten grains of the essential salt, would be more than the stomach would retain, or the patient persuaded to swallow. From these great objections this salt is exempt." Its being soluble in small quantities of either vinous or aqueous menstruums, and its convenient form for being used in pills, renders it extensively applicable in cases where the powder cannot be duly administered.

This substance, like some of the deliquescent salts, absorbs moisture greedily, and after exposure to a moist atmosphere, it parts with its yellowish colour and vitreous appearance, and assumes, first, a gum-like; then a syrup-like consistence of a dark brown colour. The change thus wrought on this article by the admission of moisture, while it does not materially injure it as a medicine, destroys in a measure that characteristic appearance, by which its genuineness is to be judged of.

I find that a solution of the above concrete or salt, in the same quantity of water which originally held it in solution, produces, on the addition of red sulphate of iron, the same succession of dark and green colours that were observable in the infusion before evaporation.

An infusion of galls, added to a fresh cold infusion of yellow bark, produced a copious white precipitate. A solution of the salt of equal strength with the infusion, on the addition of galls, let fall a precipitate precisely similar in colour and quantity.

On the addition of a solution of gelatin to a fresh cold infusion, little or no precipitate could be distinguished. The same result followed the addition of gelatin to a solution of the salt.

A solution of red sulphate of iron (fifteen grains in one ounce of water or spirit) when added to eight times its quantity of the tincture obtained from the residuum of bark after maceration in water, and which afforded the concrete in the last column, gives it a dark green colour and turbid appearance. When the concrete is redissolved in the original quantity of spirit, the solution undergoes the same change on the addition of the chalybeate.

The above tincture affords, on the addition of either infusion or tincture of galls, a very small grey-coloured precipitate. A similar precipitate is deposited by a solution of the concrete on the addition of galls.

When water in large proportion is added to the tincture above mentioned, a floculent precipitate is formed. When the concrete is redissolved in proof spirit, and water is added, a precipitate is likewise formed, similar to the other in appearance and quantity.

The experiments made on these concretes, being calculated to ascertain the existence of several of those principles on which the virtues of the bark are supposed to depend, prove, that they contain those principles unchanged. As to sensible properties, they retain much of the taste peculiar to Peruvian bark. It will be perceived that the principal difference between the mode of preparing this and the common extract of the bark, depends on the application of moderate heat in preparing the one, and the higher degrees in the other. It is now generally admitted, that the great heat which is usually employed in making the extract by long boiling, destroys that just combination of its constituent principles on which its medicinal power depends. Does the great heat employed, the agitated state of the boiling liquid, or both united, by favouring the absorption of oxygen, produce this effect?

Among the causes which have lessened the usefulness of the bark, its misapplication and depreciated quality have been noticed. Another cause of miscarriage has been owing to the small quantity administered. It is with this, as it is in the use of many other permanent stimulants (mercury, for instance) that no beneficial effect is experienced until the full quantity is given which corresponds with the lost excitement. If, instead of giving infusions and decoctions in the frivolous quantity of two or three spoonfuls, or the tincture by tea-spoonfuls, as is too frequently the practice even in cases of urgency and danger, the powder were to be administered in the dose of a drachm or more every hour or two, or if it were injected into the lower bowels in still larger quantities, less frequent complaint would be made of its inefficacy. Not a few are the number of cases wherein the invigorating power of the bark is most imperiously demanded, where the requisite quantity of the powder cannot be retained on the stomach. In nearly every case thus circumstanced, the desired effect may be expected from liberal injections of this substance. I have witnessed more sudden and complete effects from bark exhibited in this way than from any other. (This may be owing to the large quantity used.) One ounce or more of fine powder, incorporated with twelve ounces of starch mucilage and fifty or sixty drops of liquid laudanum (where that is admissible) will generally be retained, especially if mechanical pressure to the rectum be made for the first half hour. This quantity of bark, with thirty or forty drops of laudanum, may be safely repeated three or four times in every twenty-four hours.

Intermitting and remitting forms of fever generally affect aged and infirm people dangerously, and when they rage epidemically are often noticed to be malignant, and more or less mortal, to persons of every age and description. In cases like these, and many others, where the stomach cannot receive the requisite quantity without the debilitating effect of vomiting, it is, that bark injections become indispensable to the safety of the patient. I have known very many persons, from these and other causes, affected with coma, vomiting, cold extremities, paralysis of the sphincter muscles, almost imperceptible pulse, &c. speedily restored by the liberal use of bark in this way. Repeated experience furnishes the most conclusive evidence to my mind, that the human system is not less speedily and effectually acted upon by bark, in the appropriate quantity, through the medium of the lower intestines, than it is by means of the stomach: in fact, whenever this organ has lost much of its excitability by the too liberal and long continued use of ardent spirits, opium, strong and high flavoured tea, and other high stimulants, it is manifestly the part less adapted than the lower bowels, to impart to the whole body the effect of this and some other medicines. This pleasant method of administering medicine adapted to the excitability of the part, has, it is true, been noticed by many writers on medicine; but in so incidental a manner has it been done, as to render the hint almost nugatory. Thousands have doubtless been abandoned to their fate, because the bark could not be retained on the stomach; many of whom might, by this practice, have been saved to bless the great Author of all good for giving to mankind this divine remedy.

However extensively this invaluable drug may apply to the catalogue of human infirmities, it approximates no nearer to an universal remedy than the lancet does. Like that, its power of controlling a certain description of morbid actions renders it equally mischievous in those of a contrary state. Although extensively useful in intermitting forms of fever, it cannot be universally so. The intermitting and remitting forms of fever, as well as others, are now known to assume, in different places and seasons, and with different persons, very various and contrary characters. Whenever fevers of this description are characterized by a hard pulse, inflamed eyes, great pain in the head, stomach or intestines, with oppression of the breast, &c. no practitioner would think of curing them without having recourse to the lancet, and other depleting remedies. When symptoms of a contrary nature supervene, the lancet would be as prejudicial as the bark beneficial.

It is worthy of paticular remark, that intermittent fevers rarely, if ever, assume at their commencement the tonic or atonic character on which our indication of cure are subsequently formed. Those which might, by a repetition of paroxysms, assume in their further progress the tonic or inflammatory diathesis, are as certainly cured by the early use of the bark, as if no such predisposition to phlegmasia had existed. It is in this early stage of the diseases, before the morbid actions of the system are habitually confirmed, or the glandular secretions are materially vitiated, that it is best subdued by the bark. The evacuation of the primæ viæ so generally premised to the use of the bark, is here to be dispensed with, on account of the delay which it occasions, and because the state of the secretions does not require it. In that intermediate and doubtful state of the excitement which attends not only these, but some other periodical diseases, we may venture, with greater confidence, on the borders of the inflammatory state, with this remedy in our hands, than with any other tonic of the materia medica.

The yellow is that species of bark which is now most generally preferred in the United States. It is on this account, and because the best descriptions of this kind are thought to be equal, if not superior to those of other colours, that I have latterly had this kind powdered, and put into market. The superfine powder is obtained from that variety which we have denominated first quality of yellow (No. 5.), in the proportion of about two parts of the superfine to one of the remainder. This preparation has been more successfully employed (given in two thirds the usual quantity) during several years of my practice, than any other heretofore made use of, and the testimonials of many other practitioners to whose judgment it has been confided, being equally in its favour, it is not on slight grounds that it is recommended to your notice.

The first account of the preparation and uses of the concrete or essential salt of the bark was noticed in the London Medical and Physical Journal, vol. vii. The more particular remarks of Dr. Reece, and other physicians with whom he has corresponded on the merits of this article, go far to encourage its use. I have given it a few trials in cases of distinctly marked intermittents, in the dose of ten grains to adults, in the form of pills, every two hours, during the intermission, with uniform success; and in several cases of chronic debility which have recently been under my care, it has had the happiest effect, when given by itself, or in combination with chalybeates. In consequence of the evidence thus furnished in favour of this preparation, I have been induced to construct an evaporating furnace for the purpose of preparing it in the large way; and should the result of the further experiments which are now in progress not tend to invalidate the opinion already formed of it, I propose to supply those agents who may have charge of the superfine powder, with this article likewise.

After more than twenty-five years observation on the effect of bark, as a medicine, in different situations best calculated to give it an extensive trial, as well as ten years attention to the different ways of pulverizing and otherwise preparing this substance for use, it may not be thought arrogant to lay claim to confidence in regard to the facts and principles herein contended for. A few experiments, if made with attention to accuracy, as is here pointed out, and (what is a surer criterion) a few trials made with these preparations in well adapted cases of disease, may determine them satisfactorily.

Should the above remarks and observations, by exciting a spirit of further research on the subjects to which they relate, prove in that or any other way useful, it will answer the wish of

Your obedient servant,

CLARK SANFORD, M. D.

Greenwich, Connecticut, June, 1811.

* * *

Further Remarks on the same Subject. By the same.

I will now state the result of subsequent observations made respecting the medical use of the essential salt of bark, describe more particularly the manner of preparing it, and add some further particulars by which a cincona may be most readily and accurately distinguished from every other vegetable substance.

The diseases which occurred during the late summer and autumn, furnished considerable opportunities for using the essential salt of bark: the effects observed in the treatment of intermitting fevers, correspond nearly with the opinion given of this preparation in the foregoing letter; it ought, however, to be remarked, that fevers of this description, which were checked by this extract, were observed to recur more speedily after it was discontinued, and generally in a greater proportion of cases, than where the powder was liberally employed. I readily concur in opinion with Vauquelin, that the effects produced by the essential salt, are not proportional to the bark from which it is taken; nevertheless, I think that this preparation ought to be deemed of considerable importance. I have several times witnessed its great efficacy in the last stages of cholera infantum, as well as in other infantile complaints, of both acute and chronic forms. As it is soluble in nearly the same quantity of water as sugar, it is remarkably convenient for the use of children. It sets easier on the stomach, and is less liable to prove cathartic than the powder. Owing to its small bulk, and convenient form for exhibition in pills, it can be used in a disguised form, where the powder would be rejected. I have known it produce very happy effects given in this way, where the naming of bark occasioned the most distressing loathing. This extract, when dissolved in much water, and kept in a warm situation, is speedily decomposed, and becomes mouldy; therefore aqueous solutions of this substance ought to be used (especially in summer) within two or three days after being made.

In the preparation of the concrete or essential salts of the bark, great care and delicacy of management are required, through every stage of the process: the cold infusion ought to remain on the pulverized bark until the liquor is very transparent, say from twenty to thirty hours; the second infusion from thirty to forty; and the third from forty to fifty hours.

The clear liquor is then to be decanted, and after being filtered through several folds of fine flannel or bibulous paper, is to be evaporated in earthen plates, in a degree of heat which is not painful to the hand of the operator, until about one half the quantity is reduced: at this point of evaporation, the liquor ought to be decanted with a steady hand into other plates, to free it from an earthy looking sediment which is thrown down by the heat. This part of the operation must be repeated once or twice more, or as often as further sediment appears. When the liquor is reduced to the consistence of thick syrup, the plates containing it are to be removed from the bottom of the furnace, and placed above on slated scaffolds, until the salt is dry and breaks up in brilliant scales. It is only in a cool and dry atmosphere that this substance can be removed from the plates in a dry state, by reason of its great eagerness to absorb moisture.

Infusions of bark undergo the putrefactive fermentation during the summer months in so short a time, it frequently will happen that the first infusion only can be used; for this reason, as well as on account of the deliquescent property of the salt, the temperate seasons are to be preferred for manufacturing it.

I have hitherto confined my operations to the preparation of the dry extract, by slow evaporation, according to the plan of the Count De Garage, its inventor, and which that author and others have denominated the essential salt of the bark. Should opportunity permit, I propose hereafter to attempt the preparation of the salt in the white and crystalized form, as described by Vauquelin to be done by Dechamps in France; I design likewise to prosecute inquiries, already begun, respecting the properties and use of the dry extract of bark, which is obtained by means of alcohol; the result of which trials will in due time be submitted. Until the experiment of M. Vauquelin fell into my hands, I was persuaded that every bark, which when treated with water, precipitated galls and not animal glue, was changed by solutions of iron, to a darkish green colour, and which imparted little or no colour to cold water, was a cincona, and that such vegetable substances as did not afford these precise appearances, when treated in the same way, contained little or none of the cinconic principle. I was led to this conclusion, because it was supported by some respectable authorities, and because every genuine bark, of known and approved virtues, of whatever colour, which I had examined, afforded these phenomena in a conspicuous manner; and because other barks, resembling the Peruvian in their sensible properties of bitterness and astringency (of which kind are the barks of the cornus florida and salix alba) will not precipitate galls and turn iron green, but will form a copious precipitate with gelatin. The angustura, and the lately introduced bark under the name of alcornoque, are the only two kinds which I have found, when tested as above, to agree with the Peruvian bark. These being of the growth of the same country with the bark, and being equally tonic and perhaps febrifuge too, it is not unlikely, that by further experience, they will be found to belong to the family of the cincona. If the chemical experiments of Vauquelin are correctly stated, and those barks which he found to precipitate isinglass, and not galls, and also give out to cold water high colours, are barks containing the febrifuge principle, as he intimates, it would seem that the tests here contended for, were made doubtful; or that a highly predominating astringency was in these particular barks held in combination with the cinconic principle.

But as this author has furnished no satisfactory evidence derived from actual observation of their medicinal effects on the human system; and as many other astringent barks which possess no febrifuge power, agree with them in chemical results, it may with propriety be doubted whether these supposed Peruvian barks have a stronger claim to our confidence as febrifuge medicines, than the barks of the oak, persimmon, and numerous others. I think it may therefore be safely assumed, as before stated, that every bark whose cold infusion is not higher coloured than a feint orange, which forms a copious whitish precipitate with the infusion of galls, little or none with solutions of isinglass, and which is changed, by a solution of red or green sulphate of iron, to a darkish green (not black or brown), is the true cincona. The best red bark, marked No. 1, and the first quality of yellow, No. 5, produce, when treated alike, results so similar to each other in regard to appearance of precipitate and colour of fluids, that they are not in this way easily distinguished. The long time required for the powder of the red bark to subside from its containing water, probably has occasioned some mistake as to the colour of its cold infusion.

It is highly desirable that some means should be discovered of ascertaining the medicinal power of different kinds of Peruvian bark with greater certainty than can be done by the taste and other senses; and more readily than by observing the quantity of extract which they afford, or by the still more tedious means of observing their effects on the diseased patient. I had hoped that those agents which serve to identify the principles sought for, would likewise show the quantity of those principles: but I have not as yet been able to ascertain the quantity by means of those agents which show the existence of the principle: For infusions of bark, whether they be made strong or weak, afford, with galls, nearly the same quantity of precipitate; and weak infusions, when united with iron, do not differ visibly in colour or otherwise from stronger ones treated in the same way. This branch of our inquiry in particular requires further investigation, and I hope it will be prosecuted by those who have more leisure than commonly falls to the lot of a country practitioner.

From the Edinburgh Review, or Critical Journal.

The Bakerian Lecture.—On some of the Combinations of Oxymuriatic Gas and Oxygene, and on the Chemical Relations of these Principles to inflammable Bodies. By HUMPHREY DAVY, *Esq. LL. D. Sec. R. S. F. R. S. E. M. R. I. A. and M. R. I.*

On a Combination of Oxymuriatic Gas and Oxygene Gas. By the same.—From the Philosophical Transactions for 1811, Part I.

WE have made a point of following this excellent chemist through his various and important discoveries, as regularly as the nature of our publication would admit; and we now resume the subject, by calling the attention of our readers to the two papers published by him in the last part of the Philosophical Transactions.

It may be recollected, that Mr. Davy's experiments had led him to doubt, or rather more than to doubt, the existence of oxygene in oxymuriatic acid, the body formerly supposed to contain it, perhaps in greater abundance than any other. Mr. Davy was inclined to believe, on the contrary, that this acid is a peculiar elementary substance, much more nearly resembling oxygene itself, than any of its compounds, usually denominated acids. The papers now before us contain further inquiries into this analogy; and although they cannot be said to advance us very far in our progress towards a clear demonstration of the proposition in question, they nevertheless contain many curious and original observations, and open the way for new and interesting inquiries. The second of the papers in particular, presents some matters of great novelty; and is more valuable, by a good deal, though far shorter than the first.

The first paper consists of a great variety of experiments and observations, many of which are very interesting, and tend to throw considerable light on the singular substance in question. But they are very miscellaneous and scarcely capable of abridgement. Our author begins with some experiments relative to the combinations of potassium and sodium with oxygene, and of potash and soda with water. When the two metallic bases are burnt in common air at a moderate heat, brownish substances are produced, extremely fusible, effervescing strongly in water, and convertible into dry alkali, by being intensely heated in atmospherical air. Mr. Davy acknowledges himself indebted to Messrs. Gay Lussac and Thenard, for the discovery that these substances are peroxides of potassium and sodium, although the exact amount of the oxygene contained in them has not been ascertained. The deflagration of potassium in nitre produces a peroxide—that of sodium does not appear to do so. The peroxides of both metals are decomposed by ignition; and the residues are hard, greyish, exceedingly dry substances,—extremely difficult of fusion,—of rather greater specific gravity than potash and soda, and, by the addition of water, heating violently, and changed into white, ordinary, fusible, potash and soda—which appear, therefore, to be hydrats of potash and soda. The greyish, hard substances, are evidently those bodies in their pure state, although we had no knowledge of them previous to the discovery of their metallic bases.

Mr. Davy examined a little more minutely this interesting point, of the union of water with those pure oxides. By distilling ordinary potash which had been ignited for some minutes with two and a half times its weight of boracic acid, which had been kept at a white heat for an hour, he obtained about 15 per cent. of water. From common soda he obtained, by a similar process, near 23 per cent.; and he then satisfied himself that the boracic acid had not contributed to this product; for that acid in great excess being ignited with the common alkalis in small quantity (all other circumstances remaining the same,) he obtained little or no water. Nor was any water at all formed by repeating this experiment with the pure alkalis obtained by the above mentioned methods. The boracic acid, he admits, contains water, do what we can to separate it by a white heat; but he contends that these trials show the impossibility of obtaining water from this acid, which has been so ignited to whiteness, by the red heat used in the experiment with the alkalis.

Our author comes, next, to the detail of several experiments and calculations, from which he deduces the following conclusion,—intelligible perhaps, but certainly not to a cursory reader very obvious, in its meaning—that "five proportions of potash, equal to 240 grains, must be decomposed to form with an equal number of proportions of oxymuriatic gas equal to 164.5 grains; five proportions of muriate of potash equal to 365 grains; and five of oxygene equal to 37.5 grains combined with one of potash, equal to 48, must unite in a triple union with one of oxymuriatic gas, equal to 32.9 to form one proportion, equal to 118.4 grains of hyperoxymuriate of potash."—It is singular how few new terms Newton invented. Whatever language he found in common use, of that he availed himself. Scarcely ever did he use a known word in a novel sense; and the language which he found ready made, he used in such a manner as to convey no inconsiderable number of discoveries the most important and the most original that mortal man has ever been permitted to make. We wish Mr. Davy would attempt to confine himself within the ordinary limits of the language; for, really, it becomes a task of no small difficulty to trace his meaning through such neology as clothes the sentence last quoted.

Our author's attention was next directed to the combinations of oxygene and oxymuriatic gas with the metals of the earths; and he found reason to conclude, that the muriates of baryta, lime and strontia, consist of the metallic bases with oxymuriatic gas.—These earths, on being heated to redness in oxymuriatic gas, give out oxygene, and leave bodies in no aspect differing from dry muriates: But Mr. Davy has not yet tried the more direct and conclusive experiment of uniting the metallic bases of these earths with oxymuriatic gas. He next examined the operation of oxymuriatic gas upon the different metals; and the most material point ascertained by his experiments is, that the oxygene given out (where any was given out) bore an exact proportion to the quantity absorbed by the metal. Thus, 2 grains of red oxide of mercury, he found, absorbed $\frac{9}{10}$ths of a cubical inch of oxymuriatic gas, and afforded 0.45 of oxygene; two grains of dark olive oxide from calomel, decomposed by potash, absorbed about $\frac{94}{100}$dths of oxymuriatic gas, and gave out $\frac{24}{100}$dths of oxygene—corrosive sublimate being produced in both cases.

The concluding section of this paper contains a number of general remarks, chiefly inferences from the facts related in it, and from other facts illustrative of our author's new and, as it appears to us for the reasons which we formerly urged, still only probable opinion, that oxymuriatic gas contains no oxygene. This doctrine unquestionably derives some additional weight from the facts detailed in the paper now before us,—but those fall short of a demonstration. The most material circumstance in this view, is the regularity with which the quantity of oxygene produced in these experiments, observes the proportion wherein that body is found in the oxides, and not the proportion wherein the oxymuriatic gas is exhibited. The apparently contrary inference to be drawn from the copious production of oxymuriatic acid, by the action of muriatic acid on oxides, and on hyperoxymuriates, is explained away by the help of Mr.

Davy's doctrine, that muriatic acid gas is a compound of hydrogene and oxymuriatic gas,—a doctrine which will probably be thought still undemonstrated, although this able and ingenious inquirer has unquestionably placed it in a point of view not far short of inductive demonstration.

An important and, we believe, an original observation, follows, upon the manner in which oxymuriatic gas acts in the process of bleaching. The illustrious discoverer of this gas, as is well known, explained its action upon the simple and wonderfully accommodating theory of phlogiston, modified and improved as it had been, and rendered still more consonant to the phenomena, by his own inquiries. He supposed the gas to act by combining with the phlogiston of the coloured bodies. The French chemists on the other hand, accounted for the fact, by supposing that a gas so plentifully furnished with oxygene, supplied the substance, which they saw, in other cases, produced powerful effects on colouring matter. Our author, who denies the existence of oxygen in this gas, has offered a third explanation, and fortified it by an experiment which well deserves notice. He filled with oxymuriatic gas a glass globe, containing muriate of lime, in a dry and powdered state. In another glass globe containing also dry muriate of lime, he placed dry paper, tinged with litmus—and after some time he exhausted it. Then connecting it with the former globe, by means of tubes and stopcocks, the litmus paper was exposed for two days to the action of the gas in the first globe; but scarcely any perceptible alteration in its colour was produced;—while paper of the same kind, exposed to oxymuriatic gas, which had not been previously kept in contact with muriate of lime, was instantly rendered white;—and litmus paper, which had not been previously dried, when exposed to oxymuriatic gas dried as before, underwent the same change, though more slowly. From this comparative trial, Mr. Davy infers, that the gas produces its effects upon coloured bodies, by decompounding the water present, and setting its oxygene loose; and he adds, that the hyperoxymuriates owe their bleaching powers, apparently, to the tendency which their metallic base has to form simple combinations with oxymuriatic gas, and thus liberate the superabundant portion of oxygene.

Upon this theory we must be allowed to offer a remark or two, though with great deference, and even some degree of doubt, lest certain considerations may have escaped us, rather than Mr. Davy. But although we presume his future experiments will clear this matter up, and add what is now wanting to substantiate the foregoing conclusions, we cannot help at present feeling the deficiency of the evidence on which it rests. The obvious want of direct experiments to determine the effects of oxygene itself, exhibited to colouring particles, is indeed common to this and to the commonly received French explanation. It may moreover be said, that the effects produced by oxygene in its nascent state may, nay, in all probability are, very different from any which the gas, when formed, would produce on substances simply immersed in it. Yet of this observation, in its full extent at least, Mr. Davy cannot well avail himself; for the superabundant oxygene in hyperoxymuriates scarcely can be said to attack colours in its nascent state. This, however, we pass over, for the purpose of coming to our chief difficulty; and with the same view we abstain from inquiring whether any measures were taken to ascertain that no other change was produced on the oxymuriatic gas by exposure to dry muriate of lime, than merely depriving it of moisture. This our principal difficulty then lies, in the supposition essential to Mr. Davy's explanation, that water remains either chemically united or mechanically suspended in the gas without any decomposition, notwithstanding the disposition of the gas to decompound it; and that the superabundant portion of oxygene remains united with the matallic bases and the oxymuriatic gas, in hyperoxymuriates, notwithstanding the disposition of those metallic bases to form simple combinations;—and yet, that the mere exhibition of the colouring matter, to whatever vegetable substance it may belong, and consequently of whatever vegetable elements it may consist, always produces the decomposition of the water in the one case, and of the hyperoxymuriatic gas in the other. There is no great difficulty, perhaps, in conceiving that moist litmus paper, when exposed to dry oxymuriatic gas, should have its water decomposed.—But if water in litmus paper is decomposed by this gas, how comes water in this gas itself to escape decomposition?—and how happens it, that the moist gas, as soon as litmus paper is exposed to it, begins to suffer decomposition immediately? And if litmus paper, and all other *coloured* vegetable substances, produce this mutual action of the parts of moist oxymuriatic gas, is it not somewhat strange, that every vegetable substance, whether coloured or not, (we mean whether already bleached or not) should not equally accelerate the decomposition,—since we have no reason whatever to believe that the colouring matter is the same in all bodies, or is something different from the body to which it belongs?—This is a point deserving further examination, and susceptible, we apprehend, of considerable elucidation by very obvious experiments.

The only other observation worthy of notice in the section which we are considering, is a correction of a common opinion, that oxymuriatic gas is capable of being crystallized when exposed to a low temperature. Mr. Davy found, by several experiments, that this is erroneous. The solution of gas in water freezes, indeed, more readily than pure water does—and hence the mistake: but if the gas be dried, and exposed in its dry state to a cold of 40° below nothing of Fahrenheit, no crystallization whatever takes place.

The first of these papers concludes with some remarks on the changes deemed by our author to be now requisite in the nomenclature of the oxymuriatic gas and its compounds. To continue the denomination originally derived from the supposition of its containing oxygene and muriatic acid, seems absurd to Mr. Davy, who considers the existence of oxygene in it, as not proved, and the existence of muriatic acid, as disproved. He therefore proposes, (after consulting some of the most eminent chemical philosophers in this country,) that it should be denominated *Chlorine* or *Chloric* gas, from its green colour; and this appellation is certainly preferable to the old one, or indeed to any other which has been given to it,—in as much as it assumes nothing with respect to its composition, but proceeds merely on the admitted and obvious quality of its colour. As many of the salts at present called Muriates are not known to contain either oxygene or muriatic acid when in their dry state, he proposes to substitute another expression for *muriate*, in those instances. Thus Libavius's liquor, instead of being a muriate of tin, is, he conceives, only a compound of oxymuriatic gas and tin, until water is added, and then it becomes a muriate of tin. Mr. Davy proposes, therefore, to modify the termination of the metallic bases, by adding the syllable *ane* to the root, in order to express their union with oxymuriatic gas. Thus he would say—*Stannane* (for Libavius's liquor)—*Argentane* (for hornsilver)—and so for the rest: and he proposes to extend this to all unions of the gas with inflammable matters. We take it for granted, that, in cases where the addition of water converts the compound into a muriate, he would have two names, one for the dry, the other for the moist body: so that, for example, stannane should denote the union of dry oxymuriatic gas with dry oxide of tin; and muriate of tin, the stannane exposed to moisture. He points out a way of denoting the proportions of oxymuriatic gas in the different compounds, by changing the *a* into the other vowels: But we need not particularize it; as, in reality, these questions of nomenclature may as well be reserved for a more advanced period of our knowledge of the subject,—a suggestion which appears not to have been absent from onr author's own mind, while preparing this part of his paper.

We come now to the second of these interesting communications, in which our author describes a peculiar combination of oxymuriatic gas with oxygene. He had observed, that the properties of oxymuriatic gas varied materially, according as it was procured from manganese and acids, or from hyperoxymuriates and acids. In whatever manner the gas is collected when procured in the former way, its properties are the same; and it is the substance of which we have been treating in the preceding part of this analysis, as the one on which Mr. Davy's experiments were made. But the gas obtained from hyperoxymuriates is only similar to the former gas, when it is collected over water;—if collected over mercury, its properties undergo a very remarkable change: It appears to be a compound of oxygene and oxymuriatic gas, with some portion of loose oxymuriatic gas—a sort of oxide of chlorine, as it should be termed according to Mr. Davy's nomenclature already mentioned, though he proposes a new one in this paper. It explodes with great facility by a heat as low as that of the hand, in transferring it from one vessel to another; and its colour is a brilliant yellow green, instead of the pale yellow of the oxymuriatic gas. By explosion it loses from 1-6th to 2-5ths of its bulk; and, giving out that amount of oxygene, resumes its pale colour. The explosive nature of this body makes it very difficult to examine it; but Mr. Davy found, that it contained about two parts in bulk of oxymuriatic acid, to one of oxygene—which is condensed to half its volume in the compound. Water takes it up in a very considerable proportion, and acquires an orange tint. When detonated with hydrogene, it produces a solution of muriatic acid, and loses a great part of its volume. Inflammable substances, for the most part, explode and burn in it;—such as do not burn at first, burn brilliantly as soon as heat is applied sufficient to make the gas explode. They generally seem to have a stronger affinity for the oxymuriatic part of the compound, than for the oxygene. With nitrous gas it produces dense red fumes, and an absorption takes place;—with muriatic acid gas, it produces a dew on the sides of the vessel and a gradual absorption, until heat is applied, which makes it go on rapidly.

From the discovery of this singular substance (the existence of which Mr. Chenevix had certainly conjectured, when he concluded from some of his experiments, that oxymuriatic gas and oxygene, in a separate form, might unite together,) several important inferences are derivable. It throws light both on Mr. Davy's theory of the oxymuriates, and various experiments formerly made upon those bodies. In the above analysis, our readers will at once recognize the origin of the phenomena which we have frequently referred to, as exhibited when sulphuric acid is exhibited to hyperoxymuriates. A deep red tinge is given to the acid; and if a drop of water be thrown upon it, an explosion, with light and heat, takes place, and the red colour is discharged. We cannot help suspecting that this red liquor is the acid loaded with the new compound; for though in one place, Mr. Davy seems to think the collecting it over mercury, or in a dry state, essential, he first of all describes the proportion of the salt to the acid as material; and leaves us rather to infer, that the abundance of the salt, in proportion to the acid, secures the production of the compound gas: which, though absorbed by water, may unite with the acid in its formation, and may also fill the vessel with its fumes. Whether the water acts by exciting heat only, or is itself decomposed in the process, we are unable, upon the facts hitherto observed, to determine. But the most singular circumstance attending the explosion of this gas is, that it takes place in consequence of an expansion and separation of the component parts of a body, instead of accompanying, like all other explosions, the union of separate substances and their condensation into a compound.

The confirmation derived from these phenomena, to Mr. Davy's theory of the oxymuriatic gas, is also worthy of our attention. "If (he observes) oxymuriatic gas contained oxygene, it is not easy to conceive why oxygene should be afforded by this new compound to muriatic gas, which must already contain oxygene in intimate union; though, on the idea of muriatic acid being a compound of hydrogen and oxymuriatic gas, the phenomena are such as might be expected. If the power of bodies to burn in oxymuriatic gas, depended upon the presence of oxygene, they all ought to burn with much more energy in the new compound;—but copper and antimony, and mercury and arsenic, and iron and sulphur, have no action upon it, till it is decomposed,—and they act then according to their relative attractions on the oxygene, or on the oxymuriatic gas."

In further illustration of this doctrine, our author adds an experiment upon oxymuriate of phosphorus. He passed that body in vapour, together with oxygene gas, through a glass tube heated to redness. The oxymuriate was decomposed; and phosphoric acid, and oxymuriatic gas were formed. Mr. Davy contends that this ought not to have happened if the oxymuriate had contained oxygene;—but that if that body consisted of phosphorus united to an uncompounded substance, containing no oxygene, then the fact is easily explained; for oxygene having a stronger attraction for phosphorus than oxymuriatic gas has, ought to unite with it and expel that gas. We will confess that this reasoning goes a very short way to convince us of the theory. The probability of that theory we have always admitted; and it derives some confirmation from the facts presented by the new compound. But the last experiment and the reasoning founded upon it, we really think extremely inconclusive, and cannot help saying that they leave the theory pretty nearly where they found it. It surely by no means follows, that oxymuriatic gas, although a compound of oxygene and some unknown base, should not have for a third body, such as phosphorus, a weaker attraction than oxygene has. Chemistry is full of such instances. Thus, carbone reduces all the metallic oxides; and again, carbonic acid is decomposed by some metals. Phosphoric acid and carbonic acid, it will not be denied, both contain oxygene. Yet carbone decomposes the former; that is, takes oxygene from phosphorus, —while phosphorus decomposes the latter, or takes oxygene from carbone;—nay more, oxide of carbone, that is, common carbonaceous matter, a body compounded of carbone and oxygene, decomposes phosphoric acid; that is, a body containing oxygene, takes oxygene from another body containing it; and, to take only one other instance, nitrous gas, containing azote and oxygene, decomposes atmospherical air, containing the same bodies in different proportions, and, for any thing we know, in a different state of combination. Surely it is not more extraordinary that phosphorus should unite with oxygene, and leave a body containing oxygene among other component parts.

So far indeed from considering the experiment above mentioned as decisive, or the argument founded upon it as proving that oxymuriatic gas contains no oxygene, we should be inclined to infer from the well known facts which we have just enumerated (and the catalogue might be indefinitely extended), that it is a very general rule of chemical affinities, that a body, by entering into composition with one or more other substances, forms a compound so different in its nature and affinities from each of its component parts, as to have, with respect to any given one of those parts, affinities quite uninfluenced by the circumstance of that part being found in it,—affinities similar to those which would subsist between the simple substances and any body wholly unconnected with them. It is consistent with this analogy, that the affinities of phosphorus, oxymuriatic gas, and oxygene, among themselves, should be the same whether oxymuriatic gas contains oxygene, or is a third body quite different from either of the others.

That Mr. Davy will hereafter—probably at no very distant period, produce ample demonstration of his doctrine by *experiments,* we have every reason to expect, if that theory is founded in truth. In the mean time we must be permitted to suspend our belief, and decline lending it to mere speculations upon probabilities, and remarks derived from analogy and loose conjectures; which, however plausible and ingenious, should rather be the forerunners of investigation, than the results of it; and should rather suggest experiments, than be made substitutes for them. Nor can we think that Mr. Davy is always so happy in his reasonings, as in the invention and prosecution of his experiments. To compass what he has done, much accurate thinking was unquestionably requisite;—without just reasoning upon his subject, he could not have invented or followed out his various conclusive experiments. To deny him such powers, would be eminently absurd;—but we observe him frequently prone to habits of less strict reasoning upon experiments invented with different objects. He sometimes argues unsatisfactorily from general principles, or the experiments of others, or even from his own, after he has well contrived them for their main object:—And if we did not mention such instances when they come in our way, we should prove ourselves unworthy of the honour which we arrogate, of recording his numerous and brilliant discoveries.

This paper concludes with a suggestion that the new compound of oxygene and oxymuriatic gas, may be denominated, from its peculiarly bright green, *euchlorine,* or *euchloric gas;* though upon this he does not strongly insist.

CASE *of* DIABETES *treated by* VENESECTION. *By* JAMES MURRAY, *Surgeon, Belfast. From the Edinburgh Medical and Surgical Journal for January*, 1812.

OCTOBER 11, 1811.—Mr. D. Murray, æt. 72, complains of insatiable thirst, obstinate constipation of the bowels, loss of appetite, great debility, frequent micturition, with a large increase in the quantity of urine, which he thinks amounts to ten quarts in the 24 hours; he is much emaciated, troubled with vertigo and dimness of sight, with a profusion of vivid colours and various figures dancing before his eyes. His skin is soft and natural, tongue parched and loaded, gums tender, eyes and ciliæ red and painful, pulse 60 and weak, spirits much depressed; he feels a hot pain often in his stomach, with a sense of weight and straitness across the breast, and an uneasy sensation in the lumbar region. The cause of his illness he attributes to anxiety and fatigue, which induced costiveness; and, about four months ago, he observed the great increase of urine after having drank spirits rather freely.

During his bad state of health, he has taken large doses of castor-oil, salts, laxative pills, and several other medicines, without any benefit; his drink for some time was lime-water.

℞ Extract. colocyn. ʒ ij.
Ol. anisi, q. s. ft. pil. xxx.

Sig. two to be taken three times a day.

℞ Acid. muriat. ℥ j.

Sig. a quart of water to be sharply acidulated with some of the drops for common drink.

12th.—Had one loose stool this morning after the pills, but finds himself no better; drank in the last 24 hours four quarts of acidulated drink, two cups of tea last night, two this morning, and a bottle of soda-water; urine during the same period, 12 quarts of a pale yellow colour: he would wish to drink much more, but abstains as much as possible, thinking that the thirst and heat of his mouth constitute his disease, and that the large quantity of urine is owing to the necessity of drinking so much. His eyes are very red this morning, and he mentions that he has often spit blood since he took the pain in his breast, and that he often passes blood in his urine, so as to tinge it of a red colour; pulse now 65 and weak, tongue white.

Nineteen ounces of blood were taken from his arm in a full stream, and the patient and by-standers were astonished to observe the artery at the cubit beginning to beat with great force, and the veins on the arms and hands filling up; he had been often bled when young, and always fainted, but he feels now stronger than for 12 months past. After bleeding he walked to his lodgings, a distance of half a mile, with unusual firmness and vigour.

13th.—Feels easier and less giddy, observes few of the coloured figures, his eyes are quite clear and spirits elevated; drank since yesterday at this time four quarts, tea included, and eat of bread and beef about 10 ounces, and half a pint of panada for supper, which he is desired to omit in future; pulse 74 and firm, tongue and mouth rather softer and cleaner than yesterday, had three motions of his bowels, but is not griped by the pills.

14th.—Slept well last night, drank during the night only one pint of the acidulated water, though he could not want less than two quarts in the preceding nights; in the last 24 hours he passed only nine pints of urine, eight ounces of which evaporated gave seven drams of extract, which was very sweet; the first cup of blood drawn the day before yesterday, did not exhibit any inflammatory coat, but the last one showed it in a slight degree.

Owing to the relief experienced after the first blood-letting, 14 ounces were now taken in a full jet from the left arm; pulse before bleeding 70, after it 68 and full; he now feels light, and says the drunkenness, as he expresses it, has entirely left his head.

15th.—Thirst and all the troublesome symptoms greatly abated, but he still suffers from the burning heat of his mouth, though in a less degree; could still drink more than he does, but finds relief by wetting his mouth. The blood drawn yesterday, exhibits the most firm and organized corium I ever saw in the most inflammatory complaint; it is contracted smaller than a shilling, and as hard to be torn as a piece of skin. Patient had two motions of his bowels last night, and had occasion to pass urine once in that time, whereas he used to rise at least four or five times in the night; urine six pints in the last 24 hours. To take three glasses of port wine in the evening, and to wear a flannel shirt.

16th.—Much as yesterday, but did not rest so well last night, thirst rather more urgent, drank four quarts in all, and passed ten pints of urine; tongue moist and clean, pulse 76; thinks himself rather weaker than yesterday; had a free stool this morning.

V. S. to the amount of 14 ounces; the first cup soon exhibited the buffy coat, but the coagulum in the other was soft and easily torn, though the blood flowed in a uniform stream; the patient, however, as usual, seems stronger, and not the least inclined to faint; after bleeding, the pulse was 80, in 20 minutes it was 88, and the skin became warm; in two hours after the pulse was again 80.

17th.—Much as yesterday, but stronger, pulse 80 and regular, thirst moderate, urine nine pints and rather lighter-coloured, appetite improving fast.

18th.—Passed in the last 24 hours only six pints of urine, of a high colour and urinous smell; pulse 75 and not so full as last week, and he thinks himself weaker, bowels regular, thirst moderate. Applr. emplast. vesic. larg. lateri dextr. Contr. omnia.

19th.—The blister remained on 20 hours and discharged well, the pain prevented him from sleeping during the night, pulse 80, drink six pints, urine six do. of a urinous smell and taste; the blister produced no strangury; eight ounces of urine gave four drams of extract.

20th.—Slept well, urine eight pints, drank in all five pints, pulse 77 and full; he coughs a little, bowels regular, tongue clean.

V. S. to the quantity of seven ounces of a florid red, no deliquium, but, on the contrary, feels rather stronger; this cup on standing had no corium, but a soft coagulum, easily broken by the probe.

21st.—Feels better to-day than he has yet been; little or no thirst, appetite nearly natural, urine only four pints of a natural colour and smell, pulse 70 and regular; he wishes strongly to return home, as he thinks himself nearly well.

21st.—Thinks himself entirely well if the blister were healed up, as it gives him uncommon pain; pulse 71; skin, eyes, and tongue perfectly natural; drink two pints, urine three do. bowels regular, appetite nearly natural; insists on riding home to-morrow, a distance of 20 miles.

22d.—Is now returning home, and thinks himself perfectly well in every respect, feels vigorous and renovated, and experiences no inconvenience but the irritation of the blistered part, which still discharges well; ordered to take with him a bottle of the acid, and a box of the pills, and to avoid wet and cold, and refrain from spirituous liquors.

I have not heard how he remains since he left town; but I regret that I could not prevail on him to remain longer, for it is scarcely possible that in 10 days he could be radically cured. I was determined, however, to publish the result of the new plan, whatever the event might be. I need scarcely add, that the success of this case is entirely owing to the information I derived from Dr Watt's late invaluable publication; for without this I could not think of bleeding an old man of 72, who was in such a state of emaciation and debility, and particularly in such a disease. I have reported this case as faithfully as possible, having nothing to support, nor any object in view, but what should actuate every individual, and in particular every member of the profession.

P. S. I should add, that I have seen several cases of diabetes before this, in which no cure was effected; they were treated as recommended by Dr. Rollo.

Belfast, October 25th, 1811.

From the London Medical Review.

Relation Historique et Chirurgicale de l'Expédition de l'Armée d'Orient, en Egypte et en Syria. Par D. J. Larrey, Docteur de l'Ecole Spéciale de Médecine de Paris, Chirurgien en Chef de l'Armée d'Orient, &c. A Paris, Chez Demonvelle et Sœurs. 8vo. 1803. pp. 480.

THE length of time that has elapsed since the publication of the volume before us, and the extracts that have already been presented to the public, do not discourage us from laying an analysis of it before our readers, when we consider the high professional, and high public importance of some of the facts which it contains, and the small number of copies of the work that have yet reached this country.

As soon as the expedition into Egypt was decreed by the Directory, M. Larrey was appointed surgeon in chief, and M. Desgenettes (who in his commission is styled Medecin en chef de l'Arm e d'Angleterre) physician in chief to the army of the East, as the French were pleased to entitle it, which was to sail under the command of Buonaparte. To these two gentlemen was confided the whole direction of the medical arrangement of the expedition. They received from the government a discretionary authority, not only to provide themselves with whatever they might desire for the completest furnishing of their respective departments, but to make up according to their own judgment, the medical and surgical establishment of that army. They applied accordingly to the professors of the schools of medicine at Montpellier and at Thoulouse, specifying the number and qualifications of the assistants they required, and desiring them to furnish them. In consequence of this application, M. Larrey procured one hundred and eight surgeons, exclusive of those regularly attached to the regiments. M. Desgenettes found more difficulty in supplying himself with a sufficient number of physicians. He applied to the school of Montpellier for six, expecting the complement to be made up by the army physicians ; but the greater part of these disappointing him, he was obliged to make a second application at Montpellier for as many more. The number ultimately under his orders he does not specify, but they were not fewer than seventeen, since he accounts for that number in the course of his work. Such was the medical establishment of the army of the East, amounting, according to Larrey and Desgenettes, to 30,000 men.

On the arrival of the army in Egypt, and the capture of Alexandria, having found an hospital in that town, he arranged his medical staff in six divisions, one being attached to each of the five brigades of the army, and the sixth, which he calls a corps de reserve of surgeons, being stationed under his own immediate direction, at head-quarters. And afterwards, at Grand Cairo, and in every place of which the French took possession, hospitals with proper medical establishments were formed. But besides these fixed hospitals, (*sédentaires,* Larrey calls them,) there were attached to the army camp-hospitals, as they may be called, (*ambulances* is his name for them,) which accompanied it in its marches, and in the field. During engagements they were disposed along the line in the most convenient situations for giving immediate assistance to the wounded; those whose wounds required immediate and capital operations being carried to the *ambulance* of the centre, where Larrey himself was stationed. At the first battle of Aboukir, in which the Turks were defeated, the French had 800 wounded, besides four generals, among whom were Lasnes and Murat.

"The most severe wounds," says M. Larrey, "were brought to the *ambulance* of the centre, I dressed them myself, and performed the necessary operations. Forty amputations were performed immediately, and with wonderful success. The wounded received on that occasion, from the surgeons belonging to the regiments and from the *ambulanos*,* the readiest and most efficacious assistance. *No one had to wait more than a quarter of an hour before his wounds were dressed.*"

At the battle of Alexandria, in which the French were defeated by the army under Sir Ralph Abercrombie, on the 21st of March, they had 1300 wounded; six hundred had been wounded in the preceding actions of the 8th and 13th of the same month; eighteen hospitals were filled with them.

"The greater number were dressed, and had the necessary operations performed on the field of battle, or immediately after they were conveyed to the hospitals at Alexandria. All on whom amputation was performed within a few hours, were cured in a short time, and without untoward symptoms."†

No account we believe has been published of the fate of our wounded after the battle of Alexandria; but we know, from certain authority, that though victorious, many of them fared worse than the conquered. Although, in some instances, they received immediate assistance in the field, yet they were removed, soon after they had been dressed, or had undergone operations, to the fleet; and as the number of wounded far exceeded the means of accommodation that had been prepared for them, much misery ensued, and many lives were lost which with due care and assistance, might have been preserved. Orders were sent for several transports to receive as many wounded as they could contain. They were soon crowded, and boats, loaded with wounded, were seen rowing about in Aboukir-bay for some time before a ship was found that could receive them; and when at length they were on board, there was, in some vessels, a total want of medical aid, as the number of staff-surgeons was not sufficient to supply the ships. It is true that the wounded derived great benefit from the attention of the navy surgeons; but, as some of these gentlemen were obliged to return to their own ships in the evening, many unfortunate men were left during the night without that attendance which their situation required.

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* Surgeons attached to the staff, and not to any particular corps.

† Page 435.

We are aware that misery is inseparably connected with war, and we do not mean to censure the individuals who compose the medical establishment of our army. The fault does not lie with them; they are willing and able to do their duty. But where the medical officers are so few, where regiments on actual service have not always their complement of surgeons, how is it possible that the immense number of wounded that are brought to their quarters can be properly attended to? We do not know what difference there is in the number of regimental surgeons in the British and French service: but we have strong reason to believe (though we do not speak from positive authority) that the proportion is far greater among the French. And we should be inclined to trace, in the greater attention which the surgeons among our enemies have it in their power from their superior number, to give to their patients, the cause of a prejudice not uncommon among soldiers and sailors in favour of the French surgeons, and which we have known to extend even to officers.

One great disadvantage of the insufficient number of medical officers is apparently that their work cannot be done in time; which in most cases of serious operation, means, that a life which might have been saved is lost. In speaking on one occasion of wounds of the limbs, from musket and cannon balls, which require amputation, M. Larrey observes, that "all wounded in such circumstances, on whom the operation is not performed within a few hours after the accident, die on their way to the hospital, or soon after entering it."* But besides the lives lost from delay, it is to be considered that where the work to be done is altogether beyond a man's power to accomplish, there are very few that will not be thrown into hurry and confusion, and do ill what their time allows them to undertake; very few who will have strength of mind in that distress to select what can be done, and apply their thoughts only to what they have decided to do. What will be the conduct of an ordinary man in such a situation? A wound is brought before him, and the life of the patient requires an immediate operation; but a dangerous operation is at all times formidable, and especially on a field of battle, if a surgeon is not within reach of the counsel and assistance of his brethren. Often from timidity, and oftener wearied out by the heart-rending importunities of the poor fellows who are waiting their turn to be dressed, he will be seduced to hope that no harm will happen from a little delay, and to put off the operation.

The only effectual means which we can imagine of supplying relief to the wounded of an army actually in battle, is by such an arrangement as M. Larrey has described under the name of *ambulance*. This plan had been suggested by the celebrated Ranby, sergeant-surgeon to George the Second, whom he accompanied in his German wars. He had witnessed the miserable insufficiency of the ordinary supply for the emergencies of an engaging army, and describes in the little treatise on gun-shot wounds, suggested by his practice in those wars, the method of which he foresaw the success, but which has never been fully acted upon in our service.

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* Page 456.

"I could wish to be indulged," says Ranby, "in offering a scheme which might, I think, be put in execution with all the facility imaginable. It is this: when the army is forming for an engagement, let the surgeons, with their respective mates of the three or four regiments that are posted next each other, collect themselves into a body, (the same method being observed throughout the line,) and take their station in the rear, according to the command of the general. Here let the wounded be put under immediate care and management. By this means they will be enabled mutually to assist each other, and to perform their duty both with more exactness and dispatch. Now let any person of common humanity reflect on the consequences naturally flowing from this manner of treating the wounded on the spot; having first formed an idea of poor creatures being actually conveying, though with all conceivable tenderness, from place to place, under the extreme misery of large, lacerated wounds, bleeding arteries, or fractured limbs; and this abrupt, preposterous removal attended with perhaps the most fatal symptoms, of which there was not perhaps at first the least appearance or apprehension. I am apt to think that such a one will not hesitate a moment in preferring this my scheme to that scene of terrible distress, which I look upon as one of the most moving that can be presented to the human eye."*

But to make these camp-hospitals effectual, of course another greater alteration in the present system is requisite:

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* Ranby on Gun-shot Wounds, page 33.

a larger medical establishment. When troops are healthy, and not near an enemy, the present establishment of the regiment is more than sufficient; but in an enemy's country, and in an unhealthy climate, it is altogether insufficient; and the medical staff of the army is likewise inadequate to the exigencies of the service. The system of warfare is now more bloody than it ever was. The greater use of the field artillery and of the bayonet in our present battles is attended with a havock, which by adding to the number of the wounded, increases the labour of the surgeons, and renders the augmentation of their number absolutely necessary. This augmentation must be adapted to the occasion. When an expedition is fitted out, in which a severe contest with the enemy may be expected, most especially when it is destined to an unhealthy climate, let government show its care of the welfare of its soldiers by raising a numerous but temporary medical staff, as was done in France. It may be done here as easily as in France, and as well. The emulation which Larrey tells us appeared in their schools for the appointment of that service, might be seen in our own, provided government could engage the confidence of the young men of the profession, and satisfy them that merit would find its own way to distinction; a trust which it could easily inspire, as was done there, by its first and leading appointments; nominating to the head and command of the whole medical establishment, not those whose names were presented by the lists of seniority and service, or obtruded by interest, but those whom their own pre-eminence in genius and knowledge had already marked out in the eyes of the country, as the legitimate claimants of the noblest office with which the country can honour the profession—the superintendance of the health and lives of its defenders.

M. Larrey professes to give an historical as well as a chirurgical account of the Egyptian expedition; he is, however, extremely concise, and very properly so, in speaking of whatever relates to the military operations of the French army. The order of their occurrence has, in a great measure, fixed the order of the professional contents of this volume, and they chiefly serve as dates in the journal which he kept in his own department. As we wish to lay before our readers whatever appears to us most interesting in point of novelty and importance, we shall insert extracts chiefly from the cases, some of which are valuable on many accounts. The chapters on the plague, and on ophthalmia, though treating of the two most remarkable complaints in Egypt, and obviously the result of much experience, seem to us to contain very little interesting matter: to these, therefore, we shall not call the reader's attention; yet we cannot help observing the singular fact, that neither M. Larrey nor M. Desgenettes, who have both written on the plague, should have been acquainted with the good effects of mercury in that disease; and yet M. L. had observed the analogy between the plague and the yellow fever of the West-Indies, and could scarcely fail to be acquainted with the latter complaint.*

The French lost a number of their wounded from tetanus; in some it proved fatal in three or four days, in a few the disease lasted longer. M. L. observed in his patients, "if not an abhorrence of fluids, at least a considerable repugnance to them;" in consequence of which it was difficult to administer internal remedies. In the case of a surgeon who died of that dreadful affection, in consequence of a wound of the face, he endeavoured to introduce into the œsophagus a tube of elastic gum; but he found it impracticable, as it occasioned convulsions and suffocation. On opening the bodies of patients that had died of locked-jaw, he found "the pharynx and œsophagus in a state of great constriction, their internal membrane red, inflamed, and lined with a viscid and reddish substance."†

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* The same analogy has been observed by Drs. M'Gregor and Chisholm.
† Page 58.

Frictions with oil, strongly recommended by some authors, were inefficacious. Mercurial frictions seemed to M. L. to aggravate the symptoms. He tried, but without any success, an application of tobacco leaves, to the wounds of patients affected with tetanus. Moxa and the actual cautery were equally unsuccessful. He was not more fortunate from the use of blisters, except when the tetanus was preceded by the cessation of suppuration from the surface of a wound; in such cases he applied blisters with advantage, and their good effects were attended with a renewal of the suppuration. The favourable result of a case of tetanus in an officer who was shot in the foot, and whose leg was amputated, after opium, tobacco, camphor, and other remedies had been used in vain, led M. Larrey to recommend the same practice in other cases; and his success supports the opinion which he advances, that in all cases of tetanus from wounds of the limbs, it would be adviseable to amputate as soon as the symptoms appear. When the officer whose case we allude to submitted to amputation, his life was despaired of; he himself wished for the operation, and it was performed as a last resource, after every other remedy had been tried in vain. The symptoms ceased almost immediately on removing the limb in which the wound was situated; even where the case in the end terminated fatally, the operation relieved the symptoms very considerably. It should seem, however, from one case, that a division of the soft parts in the vicinity of the wound, was sufficient to relieve the symptoms of tetanus, at least in the beginning. The general of division, Destaing, was shot through the middle and back part of the right arm, at the battle of Alexandria; the ball had penetrated through the muscles, and had injured the nerves of the arm; convulsive motions of the limb, considerable anguish, and some difficulty of breathing took place soon after the wound was inflicted. At the end of eight days, during which the patient had suffered considerably from pain, febrile symptoms, and loss of appetite, M. Larrey was called in. He wished immediately to lay open the wound along the course of the ball, but to this the patient refused to submit, and emollient applications were employed. However, on the following day symptoms of tetanus having made their appearance, and the patient being in a state of great anxiety, the wound was laid open by dividing the integuments, the cellular membrane and the few muscular fibres which covered the course of the ball; the lower part of the wound was likewise scarified. The operation was painful, but in two hours the patient felt considerably relieved, and at the end of two days the alarming symptoms entirely disappeared, and the patient recovered.*

During the campaign in Egypt, the operation of extirpating the arm at the shoulder joint occurred nineteen times; thirteen of the patients did well, the remaining six died of internal diseases. This operation has been performed several times in this country, and by our army surgeons; and the manner of performing it is so generally known, that we deem it unnecessary to say any thing further on that subject.

We do not know that the operation of extirpating the thigh at the hip joint was ever performed by any surgeon in this country, except by Dr. Kerr, of Northampton.† M. Larrey has performed it three times, twice in Egypt and once while he was surgeon to the army on the Rhine. His three patients died; but when it is considered that one of these survived the operation a week, at the end of which he was carried off by the plague, and that the others died after being conveyed in a very uneasy manner during a precipitate march of the army, we are not warranted, especially after the successful case that is already upon record,‡ in declaring the operation to be impracticable, and we should think very ill of a surgeon who, under the circumstances in which it is called for, should not give his patient the remaining chance of life.

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* Page 81.

† Vide Dr. Duncan's Med. Commentaries, vol. vi. p. 337.

‡ Sabatier Medicine Operat. tom. iii. p. 330.

The excision of joints is one of the most important improvements of modern surgery, and for the manner in which M. Larrey has employed that practice in the treatment of gun-shot wounds, he deserves the greatest credit. He has been able to save limbs, which, according to the rules of surgery, should have been taken off; and when the importance of saving an upper extremity, for which no substitute can be supplied, is considered, the value of his practice may be appreciated. When the arm is fractured near its upper extremity by a musket ball, it is considered by most surgeons necessary to amputate the limb, and in such cases the operation used invariably to be performed; but, says M. Larrey,

"I have had the good fortune, on ten different occasions, of superceding the necessity of the operation, by *the complete and immediate extraction of the head of the humerus and of the splinters.* I perform the operation in the following manner. I make an incision in the centre of the deltoid muscle, and parallel to its fibres, carrying the incision as low down as possible. I get the edges of the wound drawn asunder, in order to lay bare the articulation, of which the capsule is generally opened by the first incision, and by means of a probe pointed bistoury, I detach with the greatest ease from their insertions the tendons of the supra and infra spinati, of the teres minor, of the infra scapular, and of the long head of the biceps; then I disengage the head of the humerus, and remove it through the wound in the deltoid by means of my fingers, or of an elevator. I bring the humerus up to the shoulder, and fix it in a proper position by means of a sling and a bandage. Such is the operation which I performed on ten patients, in extirpating the head of the humerus; one of these died of the hospital fever, two of the scurvy, at Alexandria, and the fourth, after he was cured, died of the plague on our return to Syria. The rest returned to France in good health. The arm became anchylosed to the shoulder in some, and an artificial joint, allowing of motion, was formed in others."*

Our limits forbid our giving the details of the cases in which this operation was performed, some of which are highly interesting. In the case of a drummer, seventeen years of age, whose shoulder was struck by a four-pounder, the humeral extremity of the clavicle, the acromion and the head of the humerus were so shattered that it was necessary to remove all these parts. This young man recovered completely, and his arm became anchylosed to his shoulder.

Perhaps the most important part of M. Larrey's work is that on amputation in consequence of gun-shot wounds: this memoir, as it is called, was written as an inaugural dissertation for the degree of doctor of medicine, and its principal object is to recommend immediate amputation in all cases of gun-shot wounds which require that operation. In the year 1754, the academy of surgery offered a prize for the best dissertation on gun-shot wounds, and on the most proper time for performing amputation, when that operation became necessary in consequence of such wounds. After considerable debates, the prize was adjudged to the dissertation of M. Faure. The object of his dissertation is to recommend a delay of the operation, and M. Larrey observes, that the principles of that author have been since adopted by almost all practitioners. The practice of M. Faure is followed by the most eminent surgeons of the present day in this country; it is recommended by Mr. John Hunter, in his treatise on gun-shot wounds; the support however which it derives from the notice of Mr. Hunter, arises more from the authority of his name than from the strength of his arguments in its favour. That part of Mr. Hunter's works is, in our opinion, the weakest of all his practical writings. The propriety of immediate amputation, when its necessity is admitted to be sooner or later unavoidable, is enforced by writers whose experience gives considerable importance to their opinions. Suffice it to name Le Dran, Ranby, Kirkland, and Larrey. It would be presumptuous in us to attempt to decide a question so important and so difficult; but after considering the subject with attention, we believe it may be safely affirmed that the propriety of delaying amputation till the first inflammation is over, is far from being so firmly established as Mr. Hunter thought, and as many surgeons still maintain. The work of M. Larrey contains a collection of facts more conclusive in favour of immediate amputation than any that we have met with in any other author; and he reasons from the only experience to be relied on, viz. an experience of the comparative merits of both methods. It would exceed our limits were we to attempt to lay before the reader an analysis of that part of the work; we must content ourselves with having given our opinion of its importance, and refer to the work itself for the details.

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* Page 315.

Among the most singular wounds that occurred in M. Larrey's practice, is one of a soldier whose epiglottis was shot off at the battle of Alexandria. "The ball entered at the angle of the jaw, crossed the mouth obliquely, and went out at the opposite side, *(et sortit a la region jugulaire du cote oppose.)* The base of the tongue was grazed, and the epiglottis shot away; the patient spat it out some time after, and showed it to the surgeon who first dressed him. The patient suffered little, but he could not produce articulate sounds, and his voice, which was hoarse and feeble, was heard with difficulty." Every attempt which he made to drink was attended with a convulsive cough, and with a sense of suffocation and vomiting: he remained in that deplorable condition for four days; on the fifth day M. Larrey was called in; he introduced into the œsophagus a flexible tube, through which the patient was fed for six weeks; at the end of that time he was able to eat without the tube thick panado and thickened rice, which he formed into little balls, and swallowed with tolerable ease. His powers of speech and deglutition improved very much afterwards, in consequence, as M. Larrey imagines, of the office of the epiglottis being supplied by an enlargement of the arytenoid cartilages, and by an expansion of that part of the base of the tongue which lies next to the glottis.*

Another very singular circumstance observed by M. Larrey in Egypt, was an atrophy of the testicles, from which a number of the French soldiers suffered.

"Both testicles, but more generally one, loses in part its sensibility, becomes soft, diminishes in size gradually, and appears to dry up. The patient is not sensible of his loss till the testicles are very much reduced in size, and then they are found lying at the ring, of the shape and size of a French bean. All those to whom this accident has happened have lost all venereal desires, and become impotent, and the whole system has been weakened in consequence of that loss."†

We have already exceeded our proper limits, and yet our analysis is very imperfect; but we trust the reader will be able, from these extracts, to form some opinion of the merits of this volume. It is the work of one of the first military surgeons of the present day, raised to high rank in his profession at the beginning of a revolution, a time when merit never fails to be discovered and rewarded. To know that he has been employed in the principal campaigns of the French armies, is sufficient to give an idea of the extent of his experience. On his return to France from Egypt, he was appointed surgeon to the consular guard, and we understand that he is now body surgeon to Buonaparte. We strongly recommend this volume to the perusal of all surgeons, but particularly to army surgeons: it is to be regretted however, that copies of it cannot be readily procured in this country, and that no translation of it has yet been published.

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* Page 290.

† Page 216.

Experiments to prove that Fluids pass directly from the Stomach to the Circulation of the Blood, and thence into the Cells of the Spleen, the Gall Bladder, and Urinary Bladder, without going through the Thoracic Duct. By EVERARD HOME, *Esq. F. R. S.*[*]

HAVING on a former occasion laid before the Society some experiments to prove that fluids pass directly from the cardiac portion of the stomach, so as to arrive at the circulation of the blood without going through the thoracic duct, the only known channel by which liquids can arrive there; the present experiments are brought to confirm that opinion; but in stating them I wish to correct an error I was led into in believing that the spleen was the channel by which they are conveyed.

At the time I made my former communications, I was conscious that the facts I had ascertained were only sufficient to open a new field of inquiry; but as I might never be able to make a farther progress in an investigation beset with so many difficulties, I thought it right to put them on record. Since that time I have lost no opportunity of devising new experiments to elucidate this subject; and the circumstance of Mr. Brodie, the assistant of my philosophical as well as professional labours, having tied the thoracic duct in some experiments which will come before the Society, suggested to me the idea, that, if the thoracic duct was tied, and proper experiments made, there could be no difficulty in ascertaining whether there was any other channel between the stomach and the circulation of the blood.

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[*] Philosophical Transactions for 1811, p. 163.

With this view I instituted the following experiment, which was made on the 29th of September, 1810, by Mr. Brodie, assisted by Mr. William Brande and Mr. Gatcombe. I was unavoidably prevented from being present during the time of the experiment.

Exp. 1. A ligature was passed round the thoracic duct of a rabbit, just before it enters at the junction between the left jugular and subclavian veins: an ounce of strong infusion of rhubarb was then injected into the stomach. In three quarters of an hour some urine was voided, in which rhubarb was distinctly detected, by the addition of potash. An hour and a quarter after the injection of the rhubarb the animal was killed: a dram and half of urine was found in the bladder highly tinged with rhubarb, and the usual alteration of colour took place on the addition of potash. The coats of the thoracic duct had given way opposite the middle dorsal vertrebra, and nearly an ounce of chyle was found effused into the cavity of the thorax, beside a considerable quantity in the cellular membrane of the posterior mediastinum. Above the ruptured part the thoracic duct was entire, much distended with chyle; and on tracing it upwards, the termination of the duct in the vein was found to be completely secured by the ligature. The lacteal and lymphatic vessels had given way in several parts of the abdomen, and chyle and lymph were extravasated underneath the peritoneum.

In this and the following experiments the infusion of rhubarb was employed in preference to the prussiate of potash, in consequence of its having been found in those I formerly made, that one drop of tincture of rhubarb could be detected in half an ounce of serum, and nothing less than a quarter of a grain of prussiate of potash in the same quantity could be made to strike a blue colour when the test was added.

Exp. 2. The experiment was repeated upon a dog. In this I was assisted by Mr. Brodie, Mr. William Brande, Mr. Clift, and Mr. Gatcombe. After the thoracic duct had been secured, two ounces of strong infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine in the bladder, on the addition of potash, became deeply tinged with rhubarb. The bile in the gall bladder, by a similar test, was found to contain rhubarb. The lacteal vessels in several parts of the mesentery had burst, and chyle was extravasated into the cellular membrane; the thoracic duct had given way in the lower part of the posterior mediastinum, and chyle was extravasated. Above the ruptured part the thoracic duct was much distended with chyle; it was readily traced to the ligature, by which it was completely secured.

These experiments appeared to establish the fact, that the thoracic duct was not the channel through which the infusion of rhubarb was conveyed to the circulation of the blood, and it now became easy to ascertain, whether it passed through the spleen, by extirpating that organ, and repeating the last experiment.

On the 21st of October, 1810, the following experiment was made with the assistance of Mr. Brodie, Mr. Clift, Mr. Gatcombe, and Mr. Money.

Exp. 3. The thoracic duct near its termination was secured in a dog, whose spleen had been removed four days before, and three ounces of infusion of rhubarb were injected into the stomach. In an hour and half the dog was killed, and the urine was found strongly impregnated with rhubarb; and on examination, the thoracic duct was found to be completely secured by the ligature. Several of the lacteals had burst, but the duct itself had not given way; it was greatly distended with chyle and lymph.

By this experiment it was completely ascertained, that the spleen is not the channel through which the infusion of rhubarb is conveyed into the circulation of the blood, as I had been led to believe, and therefore the rhubarb, in my former experiments detected in the spleen, must have been deposited in the same manner as in the urine, and in the bile.

The detection of this error made me more anxious to avoid being misled respecting the thoracic duct; and therefore, although there was little probability that the infusion of rhubarb could have passed into the lymphatic vessels, which open into the blood vessels of the right side of the neck, I thought it right, before I proceeded farther, to repeat the experiment, securing the termination of the thoracic duct on the left side, and the lymphatic trunk of the right side, where it empties itself into the angle between the jugular and subclavian vein. This was done on the 28th of October, 1810, with the assistance of the same persons as in the last experiment.

Exp. 4. The thoracic duct of a dog was tied, as in the former experiment; in doing it the duct was wounded, and about a dram of chyle flowed out; the lymphatic trunk of the right side was then secured. After this, three ounces of infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine and the bile were found distinctly impregnated with rhubarb. On opening the thorax, some absorbent vessels, distended with lymph, were seen on the right side of the spine, entering an absorbent gland on the second dorsal vertebra, and the vasa efferentia from the gland were seen uniting with other absorbent vessels, and extending towards the right shoulder, where they formed a common trunk with the absorbents from the neck and axilla; this trunk was found included in the ligature. The thoracic duct was moderately distended with a mixture of chyle and lymph; in tracing it upwards, an opening was seen in it immediately below the ligature, through which the contents readily passed out when pressure was made on the duct: above this opening the duct was completely secured by the ligature. Nearly a dram of the fluid contained in the theracic duct was collected and tested by potash, but there did not appear to be any impregnation of rhubarb.

Exp. 5. The last experiment was repeated on another dog, on the 21st of January, 1811, with the assistance of Mr. Brodie, Mr. W. Brande, Mr. Clift, and Mr. Gatcombe. The dog was killed an hour after the thoracic duct and lymphatic trunk had been secured, and the infusion of rhubarb had been injected into the stomach.

In tying the right lymphatic trunk, a lymphatic vessel from the thorax going to join it was wounded, from which chyle flowed out in considerable quantity during the whole time of the experiment; a short time before the dog was killed some of it was collected, but on testing it with potash no rhubarb was detected in it.

The urine was found impregnated with rhubarb, as was also the bile from the gall bladder; but both in a less degree than in the last experiment. The lacteal vessels and mesenteric glands were much distended with chyle; and on cutting into the glands chyle flowed out in considerable quantity. Some of this was collected and tested with potash, but showed no evidence of rhubarb being contained in it. The thoracic duct was much distended; it was traced to the ligature, and was found to be completely secured.

Lymphatic vessels from the right side of the posterior mediastinum were seen extending towards the ligature, that had been tied on that side; they were nearly empty; and the trunk formed by the junction of these with the lymphatic vessels from the right axilla, and from the right side of the neck, was seen distinctly included in the ligature.

While Mr. Brodie was tracing the thoracic duct, Mr. William Brande was making an infusion of the spleen, and showed me a section of it, in which the cells were larger, and more distinct, than I had ever seen them in a dog. There was a slight tinge of rhubarb in the infusion from the spleen. A similar infusion was made of the liver; but the quantity of blood contained in it being much greater than in the spleen, the appearance was not sufficiently distinct to decide whether it contained rhubarb or not. These experiments appear completely to establish the fact, that the rhubarb did not pass through the thoracic duct, and therefore must have got into the circulation of the blood by some other channel. They likewise completely overturn the opinion I had adopted of the spleen being the medium by which the rhubarb had been conveyed, and show that the spleen answers some other purposes in the animal economy.

The rhubarb found in the spleen does net arrive there before it enters the circulation, it is therefore most probably afterwards deposited in the cells in the form of a secretion. That the rhubarb goes into the circulation is proved by my former experiments, in which it was detected in the splenic vein. The prussiate of potash is hardly to be discovered in the blood of a living animal, since the proportion which strikes a blue colour on the addition of solution of iron, is greater than the circulating fluids can be expected to contain at any one time, as it goes off by the secretions nearly as fast as it is received into the blood vessels. In a moderately sized ass more than two drams must be dissolved in the blood before its presence there can be detected.

That the fluid contained in the cells of the spleen is secreted there, is rendered highly probable, since it is most abundant while the digestive organs are employed, and scarcely at all met with when the animal has been some time without food. The great objection to this opinion is, there being no excretory duct but the lymphatic vessels of the spleen; these however are both larger and more numerous than in any other organ; they are found in the ass to form one common trunk, which opens into a large gland on the side of the thoracic duct, just above the receptaculum chyli; and when the quicksilver is made to pass through the branches of this gland, there is a trunk equally large on the opposite side, which makes an angle, and then terminates in the thoracic duct. This fact I ascertained at the Veterinary College, assisted by the deputy professor Mr. Sewell, and Mr. Clift. These lymphatic vessels are equally large as the excretory ducts of any other glands, and therefore sufficient to carry off the secretion formed in the cells of the spleen; and where a secretion is to be carried into the thoracic duct, it would be a deviation from the general plan of the animal economy, were any but lymphatic vessels employed for this purpose.

It is a strong circumstance in favour of the secretion being so conveyed, that in the last experiment, the lacteals and cells of the spleen were unusually turgid, being placed under similar circumstances, the thoracic duct being so full as not to receive their contents.

The purposes that are answered by such a secretion from the spleen into the thoracic duct cannot at present be ascertained.

Anatomie et Physiologie du Systeme Nerveux en General, et du Cerveau en particulier, avec des Observations sur la possibilitè de reconnoitre plusieurs dispositions intellectuelles et morales de l'homme, et des animaux, par la configuration de leur têtes. Par F. J. Gall et G. Spurzheim. Premier Volume. Anatomie et Physiologie du Systeme Nerveux en General, et Anatomie de Cerveau en particulier. 4to. pp. 351. Paris, 1810. Atlas en folio, contenant dixsept planches.

THE anatomy and physiology of the brain have of late engaged much of the attention of the anatomists and men of science on the continent. Alleged discoveries of facts not before noticed in the structure of this organ, together with a new mode of explaining its functions, have already formed the basis of a complete system of physiology of the brain, in which the special functions of the several parts of which it is composed are regularly determined and explained; and which includes, as essentially connected with these functions, the natural history of our faculties, our dispositions, in short, of all our intellectual and moral qualities.

Of the philosophers who have hitherto reasoned on the powers of the brain, the greater part have contented themselves with considering it as the common centre of sensation, and the general organ of the will: whilst others, without possessing any accurate knowledge of its structure, have ventured to assign particular functions to particular parts, and have fallen into the greatest absurdities. Of the numerous anatomists, and physiologists who have diligently examined this organ in various states both of health and disease, the majority, avoiding metaphysical discussions, have confined themselves to attempts at illustrating its importance in the animal system: some, and those of the greatest eminence, struck by the observance of various phenomena in the course of their researches, were convinced of the existence of different qualities, both physical and intellectual, as connected with special parts of the brain, but their notions on the subject were so confused and their judgments so much warped by the metaphysical opinions of the day, that they had the effect rather of throwing the subject into shade, than of diffusing upon it any light which might direct the attention of future inquirers. There are yet a few who think that every thing has already been done; that after the numerous dissections which have been made, and descriptions which have been given of the structure and appearances of the brain, nothing remains to be discovered, and that all farther attempts at investigating its functions must be idle and full of metaphysical uncertainty.

It is natural to suppose that a doctrine which struck at the root of the commonly received opinions, and which professed and promised so much, would be received with jealousy, and freely canvassed by those, whose self-love it so sensibly touched, and against whose theories it so strongly militated. We find, accordingly, that the opinions of Gall, on their first promulgation in Germany, whilst they had various supporters, who adopted them without examination, and bestowed on them unbounded praise, met also with strenuous opponents, who, supporting themselves by some reasonable objections to subordinate parts of the system, scrupled not to reject the whole as false and absurd. Others, unable to oppose facts by argument, broke out into lamentations on the horrible moral consequences which must follow their adoption. The greater number of the adversaries of this physiology, without at all troubling themselves to examine the subject, attacked it by ridicule, in matters of science the weakest of all the weapons of criticism.

The doctrine of Gall which excited so lively an interest on the continent, has been received so coldly in this country, that but few we believe have any accurate knowledge of the principles on which it is founded: sure we are that no one has made any attempt to investigate it. And whilst his peculiar physiological opinions have been but very imperfectly understood, and mentioned only to be laughed at, his successful labours as an anatomist, the basis of his physiological opinions, have been wholly overlooked. In France his pretensions have met with a more favourable reception; and his anatomical observations, more particularly, have been patiently and deliberately examined by men who hold the first rank amongst the anatomists and physiologists of the present day.

The doctrines of Gall with regard to the structure of the nervous system, and more particularly of the brain, totally change the common ideas and methods of investigating the subject. We should observe here, however, that he does not pretend to have discovered many new facts; the chief merit that he claims consists in the connection which he has been the first to establish between known facts, and in the general conclusions which he has deduced from them. His doctrine concerning the special functions of different parts of the brain is in many respects new, ingenious, and important. Its intent is to prove that there are in man, and animals, innate qualities and dispositions, connected with certain organs which are situated in the brain: that the brain is not so much the general organ of animal life, as the place in which the individual organs are collected, each innate quality having its special organ; that these organs show themselves on the surface of the brain, and form eminences, marked by corresponding risings of the cranium; and that from an observance of these last, we may under certain limitations, determine the existence of such organs, in a greater or less degree of developement, answering to the greater or less degree of energy in the intellectual or moral disposition with which they are connected. Its merits must rest on the number and importance of the facts which can be brought forward for its support. We shall not at present, however, enter on this part of the subject, as the volume which is to contain his whole detailed account of the opinions is not yet before the public. The judgment already passed on them has been made up from accounts published by some of his pupils, none of them entirely acknowledged by himself.

It is now more than twenty years since Dr. Gall first made known his peculiar opinions, which, by his own account, he began to form very early in life, from observations made on the dispositions and talents of his school-fellows. As he advanced he lost no opportunity of repeating, and of trying the truth of his remarks, and became more and more convinced of their correctness. Aware that any doctrine explanatory of the functions of the brain must necessarily be very imperfect, if not strictly and intimately connected with the structure of the organ, he entered on minute anatomical examinations of its substance. His opinions, as he gradually made them known, became the subject of much discussion in Germany, where they have been the cause of a warm literary warfare.

It was not long before Dr. Gall, perceiving the interest his doctrine had excited, entered on a regular course of lectures on the physiology of the brain, in which he enlarged more particularly on the existence of special organs, for different intellectual and moral qualities, and on the possibility of ascertaining their existence, by the form of the head, in the living individual. His lectures given at Vienna, were exceedingly popular, and very numerously attended; when he was interrupted by an order from the government, and prohibited from lecturing before any but foreigners. The reason assigned was, that his doctrine was dangerous in its tendency, and led to materialism, and atheism. Weak indeed, must that establishment have been in church and state, which could be seriously alarmed by the harmless doctrines of Dr. Gall. They savoured no more of materialism than any other of the numerous physiological systems of the functions of the brain, which had never probably been discussed out of the precincts of the schools, certainly had never been thought dangerous enough to require the interference of the rulers of an empire; and this too, in the nineteenth century.

Gall soon after quitted Vienna, in company with Dr. Spurzheim. They visited the principal cities in the north of Germany, giving public lectures, conversing with men of science, inviting criticism, and always diligently labouring to add to their store of facts. They every where met with a most flattering welcome; and their designs were forwarded in every way by sovereigns and men in power, as well as by the learned, and artists of every description. Among many distinguished physiologists who had now taken up the subject as one of considerable interest, we find Hufeland, Walther, Loder, Reil, and Sœmerring; the two former as opponents, the latter as supporters of most of his opinions. All of them men of extensive knowledge, and high authority in anatomical and physical science. It is by such men that the question must be finally determined.

We next find Gall and his zealous assistant Spurzheim at Paris, in which place also they have continued their labour of collecting and arranging materials for the present work. They have given repeated demonstrations of their anatomical discoveries to all who expressed any desire, or showed any intention of investigating the subject, and have left no one to complain of a want on their part either of liberality, industry, or talent. In the beginning of 1808, they presented to the Institute, in matters of science one of the first tribunals in Europe, a memoir, entitled, "Recherches sur le Systeme Nerveux en general, et sur celui du Cerveau en particulier." It was referred for examination to M. M. Tenon, Portal, Sabatier, Pinel, and Cuvier, who drew up in consequence, a most elaborate and excellent report, which contains, within a short compass, the essence of Gall's opinions relative to the anatomy of the brain, together with many valuable remarks on them by the reporters. This report has been translated, and published in the fifth volume of the Edinburgh Medical Journal, and we cannot too strongly recommend it to the notice of all such as are desirous of inquiring into the structure and probable functions of so important an organ.

The reporters, in speaking of some of the motives which induced them to undertake an examination of the memoir, say, "The consideration of the importance of the functions of the nervous system, and of the ignorance which still exists, concerning several points of its structure, notwithstanding the numerous examinations to which it has been subjected, fixed our determination. Whoever flatters himself that he is able to throw any light on a subject so interesting, and so obscure, is, in fact, entitled to be heard with attention, by a body such as ours; and we should be wanting in our first duty, if our examinations were not conducted with the greatest attention and strictest impartiality." "We invited therefore Dr. Gall and Dr. Spurzheim to our conferences. They readily dissected the brain in our presence, and we dissected it before them; we afterwards re-examined by ourselves the observations they communicated to us; lastly, we endeavoured for a time to adopt their manner of viewing the subject, and to give a clear and precise abstract of it, which we submitted to them, that we might be satisfied whether we had rightly comprehended their ideas."

Indeed, they appear to have taken every means of ascertaining what was new, or just, in the ideas of the authors of the memoir, and to have used every precaution before they formed their opinion. This opinion is, with but few exceptions, much in favour of our authors, allowing them the merit of having recalled to our attention, and again demonstrated neglected facts, of having discovered new ones, and of having drawn some just and important conclusions from both.

The memoir above alluded to has since been published by its authors, with some remarks on this report. It is confined wholly to anatomical investigations.

We confess that it has excited our surprise, to find so little notice taken of the memoir, or of the report, by the men of science, or by the anatomical teachers of this country, the latter of whom still go on slicing the brain as they were wont to do. It has always appeared to us, that the method of demonstrating the brain at present commonly employed in our anatomical schools, is defective in every sense. What idea could possibly be formed of the appearance or structure of the thoracic or abdominal viscera, if the dissection were performed in a similar manner? Who could divine the functions of the heart or stomach from a demonstration of thin horizontal slices? The principal effect of this practice is, to puzzle the student with hard names of parts imperfectly shown, the structure and relations of which are neither pointed out nor sufficiently explained. Add to this, from the manner in which the subject is generally treated, the student is soon impressed with the idea that a minute knowledge of the anatomy of the brain can never be of much service to him in the practice of his profession, and that any time spent in acquiring it is but so much thrown away.

Of the work now before us the first volume only has been received in this country: we believe it is the only one yet published. It contains a very detailed description of the nervous system, and of the brain; and is accompanied by a folio volume of plates, executed in a most masterly and finished manner. The second volume will contain the general principles of the physiology of the brain; and the third the doctrine of the different organs.

It has been objected to Gall that he cautiously abstained from printing any of his opinions, and that he depended for popularity on the flattering accounts of prejudiced and enthusiastic pupils. To this he answers in the preface to the present work: "Every one expressed a desire of seeing me put to the press my physiology of the brain; I was much inclined to do it, but from day to day a wider field opened to my view, and questions and problems continually multiplied. It is for these reasons, that, notwithstanding repeated solicitations, and many keen provocations, I always endeavoured to put off the time of publication. If I am enabled to recompence the reader by a more abundant harvest of observations, and by more positive conclusions, it is owing to this intentional delay, and to my regard for science."

A considerable part of the present volume is occupied by a long desultory preface, and an introduction. The former notices the various labours of the authors, and the obstacles which they had to overcome; and touches slightly on some of their physiological opinions. It betrays here and there an inclination in the composers to give us some fine writing, but at the same time contains many philosophical views of the functions of the nervous system.

"In this volume our principal object is the anatomy of the brain. But in order to give a connected view of the whole, and to enable the reader to understand more readily the laws of this, the most noble part of the nervous system, we shall begin by treating of the more simple and lower order of systems, the ganglions and plexuses of the abdomen and thorax, and the nervous systems of the vertebral canal and of the senses. As we do not confine ourselves to give merely a detail of the mechanical structure of the brain, but connect with it our views of its physiology, we shall also join to our anatomical description of the inferior nervous systems some account of their functions."

"It has been said indeed that our doctrine on the structure of the brain has no necessary and immediate connection with our doctrines of its functions, and the influence of the proportionate size of its different parts; that all we have advanced on the structure of this organ might be equally either true or false, without thereby affecting the conclusions that may be drawn as to our physiological opinions. But this is separating anatomy from physiology; it is destroying all the relations between organs and their functions. It is certain that any doctrine of the functions of the brain would be false, if in contradiction to its structure: at the same time we do not pretend to explain any faculty by anatomical structure. Anatomy may serve to confirm, and to explain physiological truths; but it is seldom that it leads to a knowledge of functions, especially where the brain is the subject, the parts which have so much apparent resemblance to each other. Whoever is convinced that the structure of the parts of the brain has a necessary and immediate relation to their functions, will perceive that it is natural to unite these two objects, and to consider them as forming parts of the same body of doctrine. Since we attach so much importance to an exact knowledge of other organs, why not still more to that of the nervous system? Can there be in all the domains of natural history a study which concerns us more nearly than that of the organization of the brain?

"In anatomy our principal aim is to make known the organization of the nervous system, and particularly that of the brain; directing our attention in the first place towards the discovery of the organic laws of the nervous system; and in demonstrating them to set off from the more simple systems, and to ascend by degrees to the more complicated. In physiology we endeavour, indeed, to determine the primitive faculties of the mind, the situation, and the external marks of their special organs; but however important a knowledge of these may be to the philosopher, or the physician, we are far from confining ourselves to this single object, only regarding it on the contrary as one of the best means of ascertaining with exactness the functions of the brain and of its several parts. Consequently we direct our attention in an especial manner to researches on the faculties of irritability, of sensibility, of voluntary motion, as well as on the functions of the senses, the will, or thought, as far as any of these qualities is dependent on conditions of matter. On every occasion we take into our account, education, morals, legislation, the art of healing in general, as well as the knowledge and treatment of mental and moral affections."

We have allowed the authors to speak for themselves, that our readers might see the extensive route they had marked out, and the importance of the subjects which they intend to examine. We must excuse a little vanity in the following account of the opposition made to their doctrines, and of their final triumph. It is but a small part of what they say on this subject; we have many bitter complaints of the prejudices and jealousy of rival anatomists, as well as self-congratulations on the impregnability of the principal parts of their doctrines.

"We have hitherto taken notice of all the objections made to our assertions, which have come to our knowledge, and we shall always do the same. The only thing we regret is, that our antagonists do not take time to understand our doctrine before undertaking to refute it. The chief part of them attack rather some absurdities which have been attributed to us than our real opinions; a manner of proceeding which betrays motives of no honourable source rather than a zeal for truth. Any discovery, however, would remain very incomplete, if it obtained universal assent too readily. For the success of its author, nothing is required but that he live under a government which does not believe that the safety of the universe may be endangered by publishing natural truths. It is the coward only who is intimidated by difficulties and trifling persecutions. The man who is conscious of being the supporter of a new truth, feels a new spur in every difficulty, in every obstacle a new subject for triumph. It is only by such opposition that he is enabled to view the subject in all its bearings; it is only by resolving doubts, and answering objections, that he arrives at the point of placing the principles of his doctrines in security against the reach of any attack. It is thus that we have succeeded in placing in the rank of confirmed and demonstrable knowledge, the discoveries which we have made on the structure, the arrangement, the connections, and the relations of different parts of the brain; on the use of its two substances, as well as on the origin and direction of the nervous fibres. We believe that we have introduced order, unity, and life into a study in which hitherto there has been so much disorder. Where nothing was seen but mechanical forms, we shall show the material apparatus of the functions of the mind. Without presuming to understand the essential principle of our faculties, we shall demonstrate the poreal conditions on which these faculties depend."

We were struck by some true and eloquent observations on the peculiar advantages and opportunities enjoyed exclusively by medical men, of studying human nature; and on the probable good which might be hoped for from their experience, in regulating civil institutions. It is by such views that we are led to value more highly, and to allow more readily the importance and utility of an honourable profession.

In the introduction we have an historical account of all the various opinions which have been held on the physiology and anatomy of the nervous system; some observations on the importance of studying its functions, and on the obstacles which retarded this branch of knowledge. In the concluding paragraph we find a review of their proceedings, which we insert, in order to give our readers a favourable impression of the merits of men who have laboured so indefatigably. Without particularizing their anatomical investigations, they say—

"In order to remove every obstacle, and employ every means within our reach, we have combined with our previous researches, and with the fruits of many years labour, all the information we have been able to collect in our travels. Our intercourse with a great number of distinguished men, and an observance of their predominant qualities, have furnished us with many luminous ideas. We have profited by the observations made by our auditors, and by our friends, and by the objections made by sceptics, and by our adversaries. It is thus that we are arrived at the knowledge of many difficulties, and many defects in our doctrine. We have, at the same time, studied without ceasing the organization of men of great talents, and that of men of low talents, in order, by the comparison, the better to hit on the difference between them. We have collected innumerable facts in our visits to schools and the great establishments for education, in the hospitals for foundlings and for idiots, in the houses of correction and in prisons, in courts of justice and at the scaffold. We have added very largely to our collection of busts, and of the skulls of men and animals. We have examined a great many anatomical cabinets, &c. And, lastly, we have compared our opinions and our maxims with those of ancient and modern authors.

"After the employment of all these precautions we hope to have the credit of being thought sufficiently prepared to treat of the anatomy and physiology of the nervous system, as well as the history of these sciences. We shall attach ourselves more particularly to the brain, as being of the most delicate and most perfect organization, and as the immediate instrument of the soul. These labours appear to us as of the greater importance, as it is in this part that the knowledge of anatomists, physiologists, and philosophers, is the least advanced. For the same reason we think we have some claim to the indulgence of our cotemporaries, and of posterity, for the errors and imperfections that may be discovered in our works. They will not be attributed to a want of application, to the omission of any necessary means, but solely to the incompetency of our talents, and to the shortness of life. We shall ourselves point out some deficiencies in our doctrine; without doubt our successors will discover others, which at present we can see but in the distance."

Hitherto we have done nothing but introduce our authors, and we have been thus copious in our observations, to enable our readers to form a just estimate of their pretensions and labours. We have been anxious to vindicate the name of Dr. Gall, from that ridicule with which in this country, it is generally associated, to represent him not as the idle dreamer of craniology, but the author of deep and erudite researches into the anatomy and physiology of the brain.

It is so rarely our lot to meet with a book containing accounts of discoveries in the structure and functions of any part of the human body, that we believe many of our readers would agree with us in entering at length into an examination of the important contents of the present volume. But it would be fruitless to endeavour to discuss the many subjects of which it treats, within the necessarily short limits of a Review. It contains such detailed descriptions of different anatomical appearances; so many physiological disquisitions; so copious a history of the opinions of every eminent anatomist, as well as those more peculiarly their own; and such elaborate answers to the objections made to many of their conclusions, that it will be impossible even to give a view which shall include every point. We must confine ourselves to noticing the most prominent, and most interesting features of the discoveries and opinions contained in the present work.

Our authors begin with the examination of the intercostal or great sympathetic nerve, because, in the more perfect classes of animals, the ganglions and plexuses of the abdomen and thorax, may be considered as forming the system of an inferior order of functions. Their opinions, with regard to the origins of this nerve, will wear no semblance of novelty to such as are acquainted with the writings of later physiologists. The opinion which regarded the brain as the origin and point of departure of all the nerves of the body, either immediately or by means of the spinal marrow, has been gradually departed from, in proportion as a better acquaintance with comparative anatomy, and a more accurate mode of reasoning in physiology, have shown its inconsistency. Many anatomists have remarked, in particular, the impropriety of deriving the great sympathetic nerve from the brain, and have considered the ganglions as so many separate origins of it. Bichat has entered more at large on the same subject, and in establishing the ganglions as so many nervous centres, possessing each an independent action, or influence, exerted on the nervous filaments passing from them to different parts, has given us occasion to admire the clearness of his conceptions, and the expressive language by which his theory is at once supported and adorned. To strengthen these opinions, our authors remark, that the nervous systems of the viscera of the lower classes of animals, who possess neither brain or spinal marrow, may be looked on as analogous to similar systems in the higher classes, in whom similar functions are carried on. They correspond to the nervous plexuses of the abdomen and thorax, and to the series, more or less interrupted, of the ganglions of the sympathetic nerve: admitting this, it is demonstrated that this apparatus exists independently and of itself; and that the nervous filaments which connect it with the nerves of the head and spinal canal, are only branches of communication. We are certainly fully disposed to consider the nervous systems of the abdomen and thorax as so many distinct systems, having a reciprocal action on each other, and on the brain, and exercising different functions according as they give origin to different nerves.

The authors, throughout this work, make the greatest use of comparative anatomy. It is from analogies drawn from that source that many of their opinions originated. The manner in which it is brought forward, in the present instance, is ingenious, and the argument is well followed up. With regard to the knowledge derived from comparative anatomy in elucidating the structure and functions of the nervous system in general, we shall here for once observe, that although we must be cautious in admitting as proofs of an uncertain physiological opinion, examples taken from animals differing so widely as an acephalous molluscus and man, yet where the series of facts, with respect to the apparent organization of parts, can be clearly traced from the simple to the more compound, it must be allowed to constitute no inconsiderable proof. Nor can we see by what reasoning some physiologists are guided, who in investigating the structure of other parts of the animal system, constantly refer to comparative anatomy as a fruitful field of convincing facts, and as the surest means of avoiding errors in physiology: yet who, when the brain is the subject, will not for a moment allow any conclusions to be valid, though drawn from analogies as plain as those on which they build so much. Scarpa is allowed to bring forward the organ of hearing from the cuttle-fish, through all the different classes of animals, to explain its functions in man, We can see no reason why the same excellent plan should not be followed in the present case. There is nothing so very distinct in the brain and nervous system of man as to exclude all analogies drawn from other living creatures. Our advancement in anatomical knowledge is at once stopped if deprived of such a clue. The more simple the structure of an animal, the more easy is it to examine its nervous system; the greater the probability of making, or of confirming discoveries, than if we examined only animals of a more compound order, in which so many more parts are intermingled with one another. In man, the brain considered exclusively, is certainly more developed, more perfect in structure, than in any other creature; but we are led to the discovery of this perfection in structure by comparative examinations, in the same manner as we are led to discover the perfection of its powers. In other animals various parts of the nervous system are traced in stronger lines than in man, the whole apparatus more plainly developed, the nerves proportionally larger throughout, and their origins and terminations more distinct.

The authors proceed to investigate the functions of the systems of the great sympathetic nerve, and in conclusion give the following statement of their opinions on the nature and use of the ganglions. This we have extracted as expressive of their ideas on the origin of the nervous or medullary fibres; a point of their doctrine which has been warmly assailed, and which applied to the higher parts of the nervous system is the very key-stone on which the main part of their opinions rests.

"We have observed in the inferior classes of animals two substances forming the nervous system; the greyish gelatinous substance, and the nervous threads. The viscera are provided with collections of this gelatinous matter, and we may distinctly see nervous threads arising from them, and passing to different parts where they are lost. And as these collections of the gelatinous substance form knots or swellings, they are called ganglions. The more considerable the mass of gelatinous substance, the stronger and the more numerous are the nerves which proceed from it. We suppose that the nervous filaments are originally produced in this substance, and that they are nourished by it. The same conditions being found in the plexuses and ganglions of the higher classes of animals, we should look on these also as the origins of nerves. They are penetrated by numerous vessels, and exhibit every where the gelatinous substance, pulpy, unctuous, and of a reddish grey colour. They take different forms; and are of different consistence, according to the distribution of the cellular tissue. The gelatinous substance is sometimes generally diffused, and the nerves which arise from it form a plexus; at others, it is collected in smaller masses in the plexuses. The difference in form, consistence, and colour, shows that these qualities are not essential conditions in the nature of a ganglion; they simply indicate modifications of structure."

The second section includes the history of the nervous system of the vertebral column.

The term spinal marrow has been used in a vague manner by different authors. The natural limit, in giving a description of it, appears to be the edge of the foramen magnum. Accordingly in the present inquiry it is supposed to end at the lower extremity of the pyramidal bodies, although it is by no means inferred that the structure of the spinal marrow is changed immediately as it becomes contained within the cranium. It is to be wished that the term marrow were discarded, as it is not applicable either to the grey gelatinous substance which occupies the central part, or to the nervous fibres which form the outer part of the cord.

In common language the spinal marrow is spoken of as a prolongation, or continuation of the medullary substance of the brain and cerebellum. There are, however, numerous facts which prove that its existence is independent of these parts. In speaking of the systems analogous to the spinal marrow in the lower classes of animals, they observe that, in the worms and caterpillars there are as many origins of nerves, or ganglions, as there are rings or segments; that these knots of cineritious matter are joined to each other by nervous branches; and form a nervous cord, studded at intervals, with small tumours of various forms and size, but all divided into two parts, so that all is double. The number and size of the nervous threads proceeding from these swellings is always in a direct ratio to the size of the latter. In fishes, amphibia, and birds, the principle is the same: there is no essential difference; only the ganglions are in general closer to each other, forming with their uniting bands, a cord nearly equal in size, but swelling out in distinct knots wherever large nerves pass off; and if closely examined we may perceive throughout alternate swellings and contractions. This is very visible in the fowl, each pair arising from cineritious substances, and communicating with those on each side by connecting branches. The same law obtains in mammalia and in man, although the facts are not so immediately evident. The alternate swellings corresponding to the origin of the nerves may be seen by removing the spinal marrow, detaching the archnoid coat, and observing the profile when held against the light. The largest swellings are where the nerves of the extremities go off, but they are sufficiently visible throughout, the line being more or less undulating.

The interior structure of the spinal marrow, though often examined, does not appear to be well understood. There is evidently an anterior and a posterior longitudinal fissure. Our authors deny the existence of any lateral ones, in which they are certainly correct. The anterior fissure is more visible and larger, the posterior penetrates more deeply. The anterior fissure has also the peculiarity of being interrupted at the beginning of the medulla oblongata by the decussation of the pyramidal eminences. The two halves of the spinal marrow are united at the bottom of each fissure by a nervous layer, or commissure; the fibres in the anterior being transverse, and in the posterior directed lengthwise. They consider these nervous layers as the apparatus by means of which the two halves of this part of the nervous system communicate with and act on each other, in the same manner as takes place in the brain and cerebellum by their several commissures. This we think will be readily admitted.

It has been the common opinion that the spinal nerves are derived only in a secondary way from the spinal marrow, their original production being referred to the brain. They are here considered as originating in the cineritious substance of the spinal marrow. This substance is found in the centra parts of each side of the cord, joining "two arched streaks, which pass from the middle layer towards the anterior and posterior surface, so that the two lines of each half form two arcs, the convexities of which terminate at the median layer." All the nervous fibres follow the course of these striæ from within to the surface, throughout the whole length of the spinal marrow, so that we have on each side two rows of nerves, an anterior, and a posterior.

The nervous systems of the vertebral column serve to the brain as the instrument for voluntary motion, and the conductors for sensation. The same nerve is employed for each purpose, although by disease sometimes one power is lost and the other retained. The manner in which this is produced is not explained. The diseases of these systems are not well understood. The injury of one side of the spinal marrow is felt on the same side, and does not necessarily extend its effects to the other; there is therefore no crossing of nerves. But there certainly is a communication between the origin of each pair; which fact may throw some light on the sympathies of corresponding parts on the two sides of the body.

The existence of a canal in the middle of the spinal marrow, has been casually mentioned by several writers. Our authors give the following account of the appearance. They first observed it in examining a case of spina bifida attended with hydrocephalus:—

"In order to know positively if there existed any communication between the sac and the internal part of the spinal marrow, we divided the latter transversely in the neck: it exhibited its natural appearance. Blowing gently through a pipe on the cut surface, we perceived an opening as large as that of a common quill, in each half of the spinal marrow, separated by the commissure. They were the openings of two canals, which, by dividing lower and lower down, were found to terminate about three inches above the end of the spinal marrow, having no communication whatever with the sac. Doubtful if this appearance might be the effect of disease, we examined this part in children and in adults. We found the same structure, with this difference only, that in the adult and older children, these canals did not open so freely as in the younger subjects. The structure is most easily shown in children just born. In other respects they are alike. If we continue to blow gently into these canals from below upwards, and if, after having opened by this operation, the length of six or eight lines in the spinal marrow, we only cut off four or five, in order to maintain the opening, we may follow these canals through the medulla oblongata, the tuberculum annulare, the crura of the brain, and as far as what are called the optic thalami, in which they form a cavity as large as an almond, when blown up: they are closed at the anterior margin of the thalami, at the commencement of the corpora striata. They have no communication with the cavities of the brain."

Our authors give many sound reasons for considering these as natural canals, not formed by artificial expedients. Indeed there can be no reason to doubt a fact, which is made very apparent by gentle management. They are easily shown to within a short distance from the lower end of the spinal marrow.

At the end of this section several anatomical and physiological corollaries are introduced, containing the results of the inquiries into these parts of the nervous system. The principal points in this exposition of their opinion, and which they keep constantly in view throughout the whole work, are, that the cineritious, grey, or gelatinous substance, every where produces the nervous fibres, and that it is essentially the same in the ganglions, the spinal cord, the cerebellum, and the brain, though somewhat modified, according to the different character and functions of the nerves proceeding from it.

We are unwilling to venture an hasty opinion on these leading parts of the author's doctrines, as they are deserving much sober investigation, and appear to have been formed with much caution, and after much laboured research on their part. The propriety of using the term origin, has been often called in question, as conveying false notions with regard to the connections between different parts of the animal system. The manner in which it is employed in the present instance, has been much objected to, and the notion of the nervous fibres being produced from any part, has been ridiculed as unphilosophical. Now, it is not at all certain that every part is really formed at the same time, so that it would be incorrect to speak of one part arising from, being produced, or perfected by another. It is evident that the nervous system is essentially composed of two substances: one of a grey colour, in general pulpy and gelatinous; the other white, firmer, and formed of parallel fibres. It is also evident that we always find a collection of the first, more or less in quantity, at the beginnings of the nervous filaments. We do not say that one part is the cause of another, but we cannot see any absurdity in supposing the nervous fibres to be produced in, and dependent on the cineritious substance. We know that to follow the accepted language of physiology we must say that every part is produced immediately from the blood. Perhaps however, this will be found to be yet too sweeping a conclusion. The identity, or rather similarity in structure and functions, between the cineritious matter of the ganglions, and of the other parts of the nervous system, is the other point which our authors think they have demonstrated; but which appears to have met with as much opposition as their opinions on the production of the nervous fibres. We are aware of some objections: the strength of the argument however, is certainly at present with our authors. We wish we may not chance to meet again with physiological doctrines, less ably supported by facts and by reasoning than the present.

In the next section we have a dissertation on the differences between animal and organic life, "*la vie automatique.*" They point out some errors in the beautiful theory of Bichat, some of his opinions not being borne out sufficiently by facts. For instance, the position that the organs of animal life are double, and precisely alike in size, form, and situation, is not rigorously true. It is not correct also that the organs of animal life only arrive at perfection by frequent exercise; and that those of organic life are perfect at the first moment they are called into action. The truth is, that organs exercise their functions exactly in proportion to their developement: where the organs are complete at the instant of birth, their functions also are perfect, under whatever class we may choose to arrange them. The error has arisen from taking too partial a view of the functions of the animal system. We are, perhaps, all too fond of generalizing from limited observations on the human structure, without looking abroad through the rest of the animal creation.

Section 4. The part which immediately succeeds the nervous systems of the spine increases considerably in size as soon as it becomes contained within the cranium. It is to this part that the authors trace the "first visible origin of the greater number of the nerves of the senses, of the fifth, sixth, and seventh pairs, as well as of the brain and cerebellum. It has in common with the spinal marrow, the anterior and posterior fissures, the commissure or transverse layer of union, the internal layer of cineritious substance, and the canals made visible by blowing." It is usually called the medulla oblongata, and its limits have been variously fixed by different authors. The nerves proceeding from it are commonly termed nerves of the brain, as it appears very incorrectly. The authors call them the nerves of the head. They consider them as nervous systems, wanting in the inferior orders of animals, and designed for the functions of the senses. In the sympathetic and spinal systems, the nerves are speedily detached from the mass: here the nervous bands remain attached to the mass for a considerable space. We must be careful therefore not to confound the point of origin with the point of departure from the mass. As long as the band continues united to the mass, they think that it continually receives some augmentation from the cineritious matter contained in it. Frequently the cineritious substance is collected into a body, a ganglion, in order still further to reinforce the nervous fibres. The arrangement, according to the point of departure from the mass, is certainly therefore defective, as this is often distant from their real origin.

Our authors reckon eleven pair of the nerves, in accordance with the opinion of the best anatomists. The origins of most of these have been followed by them to the medulla oblongata. The accuracy of their conclusion is not, we think, to be doubted. Their discoveries are earnests of what may yet be done by a careful examination of parts, the structure of which is supposed to be fully known. They bear honourable testimony to the talents and labour of our authors, and should dispose us to a fair confidence in their assertion, where, from want of knowledge or address, we may not be able at once to prove them true. We can readily suppose that there are some who will consider all their pains as idle and unnecessary, who cannot see what advantage is ever to accrue from such minute investigations. It is certainly but too true, that in general, our knowledge of real structure is very inaccurate, and is wholly insufficient to guide us without error in examining the healthy or disordered functions of parts. But who is to say that we can never arrive nearer perfection, or that an intimate knowledge of organization is not the first step towards reaching it. The want of precise knowledge has ever been the greatest obstacle to improvement. For ourselves, and in respect to the present subject, we must say, that whoever has witnessed the careless routine in which the nervous system is shown in our anatomical schools, the exhibitions of melting brains and shrivelled preparations, could not be surprised, if we were to remain for ever in the dark, as to its structure and functions.

There can be no dispute about the origins of the accessory nerve, the hypoglossal nerve, the vocal nerve or nervus vagus, and the glosso-pharyngeal nerve. They bear a close resemblance in this respect to the nerves of the spine. "The following have this peculiarity, that the threads which are successively produced in the cineritious substance unite into one band before they quit the mass; which band is distinctly directed upwards, sometimes more, sometimes less visible, according as it is more or less covered by the neighbouring bands." It will not be within our limits to follow the authors through this part of their labours. The reader will find their opinions expressed with sufficient correctness in the report delivered to the Institute, referred to in our last number. There is some slight difference, but not enough to effect the general result. "It is certain that all these nervous systems originate in cineritious substance, and by no means in what is called the medullary substance of the brain. It is also demonstrated that the different nervous systems are not perfect in the first instance, but are strengthened and perfected during their course. For this purpose, they are in some instances accompanied in their passage by the cineritious substance, increasing in size the farther they are from the origin. In some cases, the nerves in their passage cross a collection of cineritious substance, the latter producing additional nervous threads, which unite to those already formed, and so on until the system is completed."

Section 5. On the difference of nerves. They consider the nerves as different throughout. There is a primitive difference in their internal structure, and an essential necessity therefore for a difference of functions. They all originate in different masses of cineritious substance, and have often an apparent difference in structure. Our authors occupy themselves, we think, rather idly towards the end of the section, in refuting at length some marvellous opinions on the subject of animal magnetism, which it should seem, are still in fashion in some parts of the continent.

In the next section they enter at great length on the functions of the external senses. The subject is handled with great ingenuity, and exhibits many proofs of originality of thought and of accurate reasoning. One of their objects is to prove the perfection of each organ of sense individually considered, and the futility of the speculations of many eminent philosophers who argue for the necessity of an exertion of the judgment, and a combined action of different organs to make the sensation perfect. We look forward to no little instruction in seeing the same extensive spirit of research, at work on the functions of the brain. Their remarks on the senses of sight, hearing, and touch, are particularly interesting, and will correct many false notions which have crept into physiology, from the philosophical writings of our most distinguished metaphysicians. This class of authors have certainly broached many incorrect, and some absurd opinions, from attending too much to the sensations in man, and from an ignorance of many points in the comparative structure and functions of man and other living creatures. If all the physiological lectures of our authors were like these, we can readily account for their popularity.

They next proceed to the examination of the cerebellum and the brain. To avoid any confusion, they consider the systems of organic life, those of voluntary motion, and of the senses, as entirely distinct; and only comprise, under the term brain, the nervous systems, which they suppose to be the special organs of the moral and intellectual qualities. It is placed in communication, and in reciprocal action and reaction with the other systems of the nerves. It no more arises from the nervous mass of the spine, than it gives origin to it. Like all the rest of the nervous systems, it is composed of two substances, the grey and the white, or of cineritious matter and nervous fibres. The fibrous nature of the last in the brain, they consider as a point in structure abundantly demonstrated. The word medullary, therefore, they wholly lay aside. In the common mode of exhibiting the organization of the brain, by thin slices, this appearance is scarcely visible, but by gentle scraping in the course of the fibres it is at all times *apparent*. Where from their superficial position, the fibres are exposed to view, without any preparation, this structure is equally apparent. The cineritious matter is variously disposed, but its nature and offices are every where the same.

Most of the nerves of the senses have their first visible origin in the cineritious substance of the medulla oblongata, "*le grand renflement*;" it is to this part also that our authors trace the first visible origin of the cerebellum. These origins form on the outer side of the medulla a large band, the *corpus retiforme*, or *processus cerebelli ad medullam*, which enlarges as it mounts, the fibres passing into the interior of the cerebellum. A short way after their entrance they meet with a mass of cineritious substance, the *corpus ciliare*, with which they become so intimately united, as to render it impossible to follow their direction. This cineritious matter is the ganglion of the cerebellum, and gives origin to many new fibres. The bands which pass out, ramify into the various subdivisions of the cerebellum. One of the principal bands passes towards the median line, and contributes with the corresponding one of the opposite side, to form the middle part of the cerebellum. This is a fundamental part found in all animals, whatever be the disposition of the brain in other respects. The lateral parts are superadded in different animals in various proportions. All these nervous fibres are covered at their peripheric extremities by cineritious substance. To exhibit these appearances, nothing more is necessary than to make a vertical section in the direction of the primitive band, so as to divide the ganglion into two equal parts.

So far they describe the nervous fibres diverging, and occupying a larger circumference. There is also another order of fibres which arise from the cineritious matter of the surface, and pass in the intervals of the fibres first described, which they cross, always converging towards the external and anterior margin, where they form a broad and thick fibrous layer. The posterior and middle filaments pass transversely through the longitudinal bands of the medulla oblongata, which go to diverge in the hemispheres of the brain. The anterior filaments lie in front of these bands, in the form of a broad fibrous layer in man. They all unite in the middle line with their fellows of the opposite side, thereby joining the two hemispheres of the cerebellum, after the same manner as the hemispheres of the brain are united by the corpus callosum, &c. This part, which now goes under the name of the pons varolii, or tuberculum annulare, is really then the great commissure of the cerebellum; and is always in a direct ratio to the size of the hemispheres, as the primitive bands are to that of the ganglion. The mammalia having smaller cerebella, have also this commissure smaller. In man, where the cerebellum is so large and so fully developed, the commissure is particularly broad, covering the origins of many of the nerves which are exposed in other animals. This commissure is not found, or there is not, to use the common language, any pons in fishes, reptiles, and birds: for these animals have no lateral parts to their cerebellum, and consequently no converging fibres to form it. But all animals have a commissure of what our authors call the fundamental part, independent of the commissure of the lateral parts, or pons. This commissure is formed by the soft and fine fibrous layers, from the superior and inferior parts of the middle or fundamental portion, commonly called the superior and inferior valves.

The cerebellum does not contain more cineritious substance, in proportion to its size, than the brain. The mistake arises from our generally making horizontal sections of the last, and vertical ones of the cerebellum; reverse them, and the brain will appear to contain the most cineritious matter. The cerebellum is not always in proportion to the brain, as to size or developement: it is not so large proportionally to the brain, in children as in adults.

In describing the brain, our authors confine themselves principally to the human subject, as the one in which the organization of the brain is the most complete, and of which the different parts can be represented in the most sensible manner.

" The brain consisting of many divisions, the functions of which are totally different, there are found many primitive bands, which by their developement contribute to form it. All these bands are composed gradually by fibres produced in the cineritious substance of the medulla oblongata *(du grande renflement.)* They are to be considered as the first visible rudiments, or commencement of the brain, having communication and reciprocal actions with the surrounding systems. Among these primitive bands, we place the anterior and posterior pyramidal eminences, the bands which proceed from the corpora olivaria, the longitudinal nervous bands which contribute to form the fourth ventricle, and some others which are concealed in the body of the medulla oblongata."

With but one exception, the bands pass into the hemispheres, keeping the same side in which they originate. With regard to the anterior pyramidal eminences, the case is reversed. Here the fibres which arise on the right side, unite in the first instance into a few cords which pass obliquely over to the left. In the same manner they pass from left to right, each cord passing over one and under another of the opposite side, forming a sort of interweaving, like basket-work. This decussation occupies a space of four or five lines at the lower extremity of the medulla oblongata. The bands afterwards pass up on the anterior surface of the medulla, gradually acquiring the pyramidal form. This appearance is very easily demonstrated, and is totally distinct from the transverse layer or commissure, visible both above and below this part, in the anterior fissure of the medulla. The authors hint that as the fibres of the anterior pyramids are the only ones which decussate, so it is only an injury of that part of the brain which is a continuation or developement of these bodies, which affects in its consequences the opposite side of the body. Injuries of the cerebellum, of the posterior lobe, and of part of the middle convolutions of the brain, would affect the same side, these parts being continuations of fibres which do not cross. This is an interesting point, and deserving of farther research.

The pyramids, as soon as they enter the tuberculum annulare, divide into many bands, which lie in a considerable mass of cineritious substance: from this many new fibres arise and join them. It is regarded as a true ganglion. The bands are so much enlarged when they pass out above, as to form nearly two thirds of the crura cerebri, "*les grands* *fasceaux fibreux.*" The transverse nervous bands seen in the tuberculum annulare, are as before mentioned, the commissures of the cerebellum. This structure may be seen by making a vertical incision curved in the direction from the pyramids to the crura cerebri. The great fibrous bands contain in their interior, a considerable quantity of cineritious matter, from which they are continually reinforced by new fibres. The nervous fibres are continued upwards, and gradually terminate in forming a nervous layer, covered by cineritious matter externally. "Thus the pyramidal bands may be traced from their point of origin, until they are perfected, and terminate in the inferior, anterior, and external convolutions of the front and middle lobes. These parts of the brain are always in proportion to the pyramids, the cineritious matter of the tuberculum annulare, and the crura cerebri in other animals, as well as in man." The same principle of structure is followed in the formation of the posterior lobe, and of the convolutions situated at the upper and internal edge of each hemisphere. The corpora olivaria they consider to be ganglions. From each there ascends a large band, joined with the posterior bands from the medulla oblongata. They cross the tuberculum annulare, and form the posterior and inner third of each crus. They receive but little addition until they enter a thick mass of cineritious matter, known generally by the name of thalamus opticus. In this ganglion is to be seen a large quantity of nervous filaments, which ascend, and at their exit upwards, unite into bands, which immediately diverge in a radiating form, and traverse another large mass of cineritious substance, the corpora striata. In this ganglion, they are again reinforced, and eventually form the posterior convolutions, and those placed internally at the upper part of each hemisphere.

The canals mentioned as existing in the nervous mass of the spine, are prolonged between the two ascending divisions of nervous fibres just described, following the course of each crus. At the part where the fibres pass out from the thalami, there is so firm a union between these and the transverse converging fibres which go to form the commissures, as to render all further separation impossible without laceration.

The thalami optici, and corpora striata, are in this view considered as true ganglions, in which the nervous fibres are increased in number and size, and are prepared by degrees to form their respective convolutions. The latter are regarded as the perfect organs, and the preceding as the preparatory apparatus. " In examining under a general point of view the manner in which nature proceeds to the primary formation of the two hemispheres of the brain, we see that the original bands are produced in the cineritious substance of the medulla oblongata, and reinforced in different places by particular masses of cineritious substance; that consequently, all follow the same laws as are observed in the nervous systems of the abdomen and the thorax, of the vertebral column, of the nerves of the head, and of the cerebellum."

All the parts of one side of the brain communicate with corresponding ones of the opposite side by transverse, or as they generally call them, by converging nervous fibres which unite at the median line. The ascending or diverging fibres have been followed into the cortical or cineritious substance, which they penetrate to some depth, making the internal part of a lighter grey than the external. How they finally terminate is not positively stated. It is thought probable that new nervous fibres are produced in the cineritious matter, intimately connected with those whose course we have been following. The existence of the converging fibres may be more readily demonstrated than even that of the diverging. They may be seen at the bottom of every convolution, decussating the latter, with which they are intermingled. As they advance inwards, they form nervous bands and layers, which go out of one hemisphere to meet corresponding ones from the other, forming the different junctions or commissures. The principal converging bands are the corpus callosum, the fornix and lyra, and the anterior and posterior commissures. The size of the different bands of reunion, is always in proportion to that of the parts they unite in the different classes of animals.

"The brain is then formed, and perfected in the same manner as the other nervous systems. The analogous parts of the two hemispheres have, as the analogous nervous systems of each side of the body, their reunions in the median line. Who in the conformity of these laws can mistake the character of truth? Can any one still prefer the arbitrary, confused, and incoherent method of studying the brain, to our method, so simple, sure, and so well followed up?"

The authors proceed to describe the different cavities of the brain, and the structure of the convolutions. The diverging fibres, after decussating the converging fibres at the outer side of the great ventricles, which they thus effectually close, divide from each other more and more, and terminate by forming a fibrous expansion. They are unequal in length, some ending immediately beyond the outer sides of the ventricles; others continuing further, the inferior having the longest course. The nervous fibres are thus lost successively in the cineritious substance, the middle ones only being continued to the extremity, and forming two layers touching each other in the middle of each convolution, slightly agglutinated by a mucous or fine cellular tissue. They are covered throughout by a layer of cineritious substance, of nearly uniform thickness. The same takes place in the cerebellum, but the divisions are more minute. Each convolution then is composed of converging and diverging fibres, and of a cineritious envelop. From the structure just described arises the possibility of separating the two layers without injuring either. The division in each convolution is easily demonstrated by blowing gently, or directing a small stream of water on the middle line. The fact is rendered still more evident if the part be hardened in nitrous or muriatic acid, diluted with alcohol, or if it be boiled in oil. If the part at which the different fibres decussate at the outer side of the ventricles, is ruptured, the whole of each hemisphere may be unfolded as it were, and made to appear as a soft bag, white and fibrous on the internal, and grey on the exterior surface. The pineal and pituitary glands are considered to be ganglions, as are also the corpora albicantia. Our authors treat also of many minor points of structure, in different parts of the brain, which we must wholly omit noticing, hoping that enough has been given to convey to the reader some notion of their principal opinions.

In this imperfect sketch, we have passed over the reasons given by Gall and Spurzheim, in support of their physiological opinions, many of which are very forcible: we have also omitted the answers given to different objections made to several parts of their doctrine. If we have not expressed any opinion as to the merits of the physiological doctrines introduced throughout this work, it is because we do not think ourselves qualified at once to decide on them, and because they rest on facts, the existence of which may be ascertained by any of our readers. The anatomical details will be found strictly correct. From a want of rightly comprehending the authors' meaning, or of sufficient address, it may happen that a superficial examiner may be disappointed in his search. The fault however will be with him. There is never any ambiguity of expression, or any confusion in the descriptions; they are all lucid, and rather diffuse. The latter circumstance we think an advantage in describing structure which is delicate, and on which there has been so much misunderstanding. Conciseness is often over-rated, and more than counterbalanced by the want of perspicuity which frequently attends it.

Whatever be the ultimate fate of their physiological opinions, anatomy must always gratefully acknowledge the labours of our authors. The great mass of anatomical information, collected in this volume, makes it highly valuable. They have, we think, taken the true route, and one unknown to, or imperfectly seen by any of their predecessors. They have made considerable advances; but the termination is yet far in the distance, and it will require long continued, and extensive research before we can hope to reach it. Whilst anatomy possesses such intelligent and indefatigable labourers as Gall and Spurzheim, and men endowed with such ample talents as Sœmerring and Cuvier, we may yet expect great things.

The subject is one of the highest interest; and the prosecution of inquiries into the organization, functions, and diseases of the brain and nervous system, is the more to be encouraged, as it is notorious that the sum of our present knowledge is trifling indeed in amount, when compared with what is yet wanting. We cannot help lamenting the apathy of many who neglect, and the absurdity of others who ridicule such labours as the present. Few, and of little import are the additions made of late years to physiological knowledge by the anatomists of this country. The appearance will be still more meagre, if contrasted with the rich discoveries made by some of the ardent anatomists of the continent. It would be out of place to enter here on the causes of this deficiency on our part: the two principal ones are the total neglect of the higher parts of physiology in our anatomical schools, and the almost exclusive encouragement given to surgical anatomy. We may well prize our chemical, but we have no cause to boast at present of our anatomical philosophers.

The plates are, without exception, the most beautiful, and the truest anatomical pictures of the brain which we have ever seen. The magnificent plates of Vicq d'Azyr, appear hard in the comparison, and they are, in many minute points, confessedly inaccurate; and from their being composed from views chiefly of horizontal sections, manifestly insufficient. The beautiful plate of the basis of the brain, by Sœmerring, the best we possessed, and by the most perfect anatomist of the present day, is not, we think, equal to a corresponding plate in the present work. They have another great merit also, besides that of being well executed, that of being well contrived to exhibit the structure of the parts they represent. [*London Medical Review.*

Description of an OURANG OUTANG: *with Observations on its intellectual Faculties. By M.* FREDERICK CUVIER.*

THE female ourang outang which formed the subject of my observations, belonged to the same species with the ourang outang described by Tulpius, Edwards, Vosmaer, Allamand, and Buffon: it is the *Simia Satyrus* of Linnæus. When erect in its natural position its height did not exceed from 26 to 30 inches: the length of the arms from the armpits to the tips of the fingers was 18 inches, and the lower extremities from the top of the thigh to the tarsus were only from eight to nine inches. The upper jaw had four sharp incisors, the two in the middle were double the breadth of the lateral, two short canine teeth, similar to those of men, and three molaria on each side, with soft tubercles. The lower jaw had also four incisors, two canine teeth, and six molaria, but the incisors were of equal size. The number of the molaria was not complete. The germ of a tooth was seen on each side at the extremity of the upper and under jaws, and it is probable that others would be produced at subsequent periods. The form of these teeth was the same with that of the molaria of men and apes in general.

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* Annales du Museum d'Hist. Nat. tome xvi. p. 46.

The hands had five fingers precisely like those of men, only the thumb extended no further than the first joint of the fore finger. The feet also had five toes, but the great toe was placed much lower than that of a man, and in its ordinary position, instead of being parallel to the other toes, it formed with them nearly a right angle. All the toes were similar in structure to the fingers and were very free in their motions, and the whole of them without exception had nails. It had almost no calves to the legs, or buttocks. The head resembled that of a man, much more than that of any animal; the forehead was high and salient, and the capacity of the cranium was great; but the neck was very short. The tongue was soft, and similar to that of other apes; and although the lips were extremely thin, and scarcely apparent, they possessed the power of extension in a considerable degree. The nose, which was completely flat, and on a level with the face at its base, was slightly salient at its extremity, and the nostrils opened downwards. The eyes were like those of other apes, and the ears completely resembled those of men.

The vulva was very small, its labia scarcely perceptible, and the clitoris entirely hid; but on each side of the vulva, there was a flesh-coloured streak, where the skin seemed to be softer than that of the other parts. Is this an indication of labia? Two mammæ were placed on the breast like those of females. The belly was naturally very large. This animal had neither tail nor callosities.

It was almost entirely covered with a reddish hair, more or less dark in colour, and of various thicknesses on the different parts of the body. The colour of the skin was generally that of slate; but the ears, the eye-lids, the muzzle, the inside of the hands and feet, the mammæ, and a longitudinal band on the right side of the belly, were of copper-coloured skin. The hair of the head, of the fore-arms, and of the legs, was of a deeper red than that of the other parts; and on the head, the back, and the upper part of the arms, it was thicker than any where else: the belly was but scantily supplied with it, and the face still less: the upper lip, the nose, the palms of the hands, and the soles of the feet alone were bare. The nails were black, and the eyes brown. All the hair was woolly, that of the fore-arm grew upwards, as did that of the arm downwards to the elbow. The hair of the head, which was harder in general than that of the other parts, grew forward. The skin, but chiefly that of the face, was coarse and rough, and that under the neck was so flabby, that the animal seemed to have a goitre when lying on its side.

The ourang outang in question, was entirely formed for living among trees. When it wanted to ascend a tree, it laid hold of the trunk or branches with its hands and feet, making use of its arms only, and not of its thighs as a man would do in similar circumstances. It could pass easily from one tree to another, when the branches met, so that in a thick forest it would never be necessary for it to descend to the ground, on which it moves with considerable difficulty. In general, all its motions are slow, but they seem to be painful when it is made to walk from one place to another: at first it rests its two hands on the ground, and brings its hinder parts slowly forward until its feet are between its hands or fore-paws; afterwards, supporting itself on its hind legs, it advances the upper part of its body, rests again on its hands as at first, and thus moves forward. It is only when we take it by one hand that it walks on its feet, and in this case it uses its other hand to support it. I have scarcely ever seen it stand firmly on the sole of the foot; most frequently it only rested on the outer edge, apparently desirous of preserving its toes from all friction on the ground; nevertheless it sometimes rested on the whole of the foot, but in this case it kept the two last phalanges bent inwards, except the great toe, which was stretched out. When resting, it sate on its buttocks with its legs folded under it, in the manner of the inhabitants of the East. It lay indiscriminately on its back or on its side, drawing up its legs, and crossing its hands over its breast; and it was fond of being covered, for it drew over it all the clothes it could reach.

This animal used its hands in all the essential motions in which men employ theirs; and it is evident, that it only requires experience to enable it to use them on almost every occasion. It generally carried its food to its mouth with its fingers; but sometimes also it seized it with its long lips; and it was by suction that it drank, like all other animals which have lips capable of being lengthened. It made use of its sense of smelling, in order to decide upon the nature of the aliments which were presented to it, and which it was not acquainted with, and it seemed to consult this sense with great assiduity. It ate almost indiscriminately, fruits, pulse, eggs, milk, and animal food: bread, coffee, and oranges were its most favourite aliments; and it once emptied an ink-bottle which came in its way without being incommoded. It had no particular times for going to meals, and ate at all seasons like an infant. Its sight and hearing were good. Music made no impression upon it. The mammiferæ are not formed by nature to be sensible to its charms, none of their wants seem to require it, and even with mankind it is an artificial want; on savages it has no other effect than a noise would have.

When defending itself, our ourang outang bit, and struck with its hands; but it was only against children that it showed any roguery, and it was always caused by impatience rather than by anger. In general, it was gentle and affectionate, and seemed to delight in society. It was fond of being caressed, gave real kisses, and seemed to experience a great deal of pleasure in sucking the fingers of those who approached it; but it did not suck its own fingers. Its cry was guttural and sharp, but it was only heard when it eagerly wanted any thing. All its signs were then very expressive: it darted its head forward in order to show its disapprobation, pouted when it was not obeyed, and when angry, it cried very loudly, rolling itself on the ground. On these occasions its neck was prodigiously swelled.

By the above description, it will be seen that the ourang outang in question, had attained a size sufficiently great for its age, which was not more than 15 or 16 months: its teeth, limbs, and powers, were almost perfect; whence it may be inferred, that it had nearly acquired its full growth, and that its life does not extend beyond 25 years.

This ourang outang arrived at Paris in the beginning of March, 1808. M. Decaen, an officer of the French navy, and brother to the Governor of the Isles of France and Bourbon, brought it from the former place, and presented it to the Empress Josephine, whose taste for natural history is conspicuous. When it arrived in the Isle of France, from Borneo, where it was born, it was only three months old: it remained three months in the Isle of France, was three months on its voyage to Spain, where it was landed, and having been two months in its journey to Paris, it must have been ten or eleven months old when it arrived, in the winter of 1808. The fatigues of a long sea voyage, but above all, the cold which the animal experienced in crossing the Pyrenees amid the snows, reduced it to the last extremity; and when it arrived at Paris, several of its toes were frozen, and it laboured under a hectic fever, brought on by obstructions in the spleen, accompanied by a cough: it refused all sustenance, and was almost motionless.

In this state it came into the possession of M. Godard, a friend of M. Decaen, who succeeded in partially restoring it to health.

I visited it almost every day while it lived; and Messrs. Godard and Decaen enabled me to add to the observations I made.

The means which succeeded in restoring this animal to some degree of health, were good victuals, a proper temperature, and, above all, cleanliness. At first, the disease was combated with tonics: bark being inadmissible in the usual way, was administered in baths and frictions; but these remedies fatigued the animal more than they relieved it, and thay were given up. The constipation of the bowels was nevertheless obstinate, and it was necessary to have frequent recourse to bathing, and this treatment was pursued till the animal's death. The desire for sucking, which it evinced, suggested the idea of suckling it again, but it refused the breast of a woman, who volunteered on this singular service. It also refused to suckle the teats of a goat. At first it seemed fond of milk, but it soon got tired of it, and of every other aliment, which was given it in succession, with the exception of oranges, which it seemed fond of to the last. In about five months the animal died; and on opening its body, most of the viscera were found to be disorganized and full of obstructions.

Such was the animal who formed the subject of my observations; and, far different from those which have hitherto been described, it had never been subjected to any particular education, and was only influenced by the circumstances in which it happened to be placed: it owed nothing to habit, nothing mechanical entered into its actions, all of them were the simple effects of volition, or at least of nature. Now that I have described the organs of this animal, and their uses, I ought to make known the phenomena which its intelligence presented: but before entering upon these details, I ought to say a word on the influence which the intellect is liable to from the modifications of our senses.

It appears to me, that some authors have made intelligence depend, much more than was just, on the greater or less perfection of the hands or fingers. Now, although the hand of an ape, and of an ourang outang, differs very little from ours, and these animals could undoubtedly make the same use of them as we do, if they were actuated by the same ideas, yet an ourang outang would no more be a man, with more perfect hands or fingers, than a man would be an ape, because he was born without arms. The influence of the senses on the mind, has been particularly exaggerated: some authors have thought, that upon the degree of perfection of these organs, the degree of the perfection of the understanding in a great measure depended. Nevertheless, it must be admitted, that several animals have senses completely similar to ours; and the description which we have given of the ourang outang, shows that this animal, which certainly is not a man, has received senses equally numerous, and at least equally delicate with ours. Besides, if we consider the real influence exercised on the operations of the understanding, by more or less delicate organs, we see that it is limited to the multiplying of ideas, in a greater or less ratio, without making any change in the manner of setting these elements at work. The most humble artisan, who has exercised his sight least, and who cannot distinguish the most striking shades of colour, will not be less of the same species with the painter, who has studied all the accidents of light, and who can recognize them in the slightest undulations of a drapery. Lastly, the understanding may have ideas, without the aid of the senses: two thirds of the brute creation are moved by ideas which they do not owe to their sensations, but which flow immediately from their brain. Instinct constitutes this order of phenomena; it is composed of ideas truly innate, in which the senses have never had the smallest share. Every thing unites, therefore, in my opinion, to convince us that it is neither in the conformation of the limbs, nor in the greater or less perfection of the senses, that we must seek the principal cause of the intellectual qualities which distinguish us from the lower animals, and even the cause of those which perhaps distinguish the animals of certain classes. The operations, the phenomena of our intelligence which characterize us, ought to proceed from higher and more potent causes; faculties, even of the understanding, or of the organ in which these faculties reside, *i. e.* the brain. Consequently, we apply ourselves much more to appreciate the use which our ourang outang made of its sensations, the results which he knew how to draw from its ideas, than to analyse these sensations themselves, or to seek for the elements, and the nature of these ideas.

All the faculties of animals concur to the same end,—the preservation of the species, and of individuals. The individual is preserved by defending himself against dangers, and by procuring what is necessary for his existence. The preservation of the species is effected by generation. It is, therefore, to preserve his existence, and to propagate, that an animal employs all his faculties, and refers all his actions; and it is with respect to defending itself against danger, and procuring necessaries for its existence, that the following observations more particularly apply. Our ourang outang was not old enough to have experienced the calls of nature in respect to generation, and to exhibit their effects. This plan simplifies the study of the intellectual faculties of brutes. Hitherto the science which has had these faculties for its object, has consisted of isolated facts, the number of which might still increase indefinitely, without increasing our knowledge, if we did not endeavour to subject them to fixed and proper rules, to regard them in their true point of view, and to appreciate their real value.

We know that the faculties of the understanding are not developed until the organs are formed: we are at liberty to suppose, therefore, that if our ourang outang had arrived at an adult age, she would have exhibited phenomena still more curious than those which we have to detail: but if we reflect that this animal was scarcely 16 months old when it died, we shall find plenty of subjects of astonishment in the observations which it afforded, and of which we are about to give an account.

Of the intellectual Phenomena which have for their Object to defend the Animal against Danger.

Nature has given the ourang outang but few means of defence. Next to man, it is an animal perhaps which finds in its own resources the feeblest defence against dangers: but in recompense it has a great facility in ascending trees, and thus escaping the enemies which it cannot combat. These sole considerations would be sufficient for encouraging the presumption that nature has endowed the ourang outang with great circumspection. In fact, the prudence of this animal is conspicuous in all its actions, and chiefly in those which have for their object to save it from some dangers. Nevertheless, its tranquil life, while under my inspection, and the impossibility of subjecting it to severe experiments in the weak state in which it was, prevented me from making many observations: but assisted by those which had been made by M. Decaen during the voyage from the Isle of France to Europe, my readers will obtain some idea of its intellectual faculties.

During the first week after its embarkation, this ourang outang evinced great fears for its safety, and seemed greatly to exaggerate the dangers of the rolling of the vessel. It never ventured to walk, without firmly grasping in its hands the ropes or other parts of the vessel: it constantly refused to ascend the masts, however solicitous the crew were to induce it, and it was only prevailed on to do so from a sentiment, or a want, which nature seems to have carried to a high degree of perfection in animals of this kind: this sentiment was that of affection, which our animal constantly evinced, and I have no doubt that it would lead the ourang outangs to live in society, and to defend themselves mutually, when certain dangers menaced them, like other animals which nature forms for herding together. However this may be, our ourang outang never had the courage to ascend the masts until M. Decaen did so himself: it followed him up for the first time; and having thus acquired some confidence in its own powers, it used frequently to repeat the experiment.

The means employed by the ourang outangs in defending themselves, are, in general, those which are common to all timid animals,—artifice and prudence: but the former have a strength of judgment far superior to the latter, and which they employ occasionally to remove enemies from them who are stronger. This was proved to us in a very remarkable manner by the animal in question. Living in a state of liberty, he was accustomed in fine weather to visit a garden, where he could take exercise in the open air, by ascending and sitting among the trees. One day that it was perched on a tree, a person approached it as if with an intention to catch it; but the animal instantly laid hold of the adjoining branches, and shook them with all its force, as if it was his intention to frighten the person who attempted to ascend, by suggesting the risk of his falling. This experiment was repeatedly made with the same results.

In whatever way we regard the above action, it must be impossible for us to overlook the result of a combination of acute intelligence, or to deny to the animal the faculty of *generalizing*. Our ourang outang, by an experiment which the wantonness of the sailors had frequently made on it, perceived that the violent agitation of bodies, which support men or animals, makes them lose their equilibrium, and fall; and it reflected that, when placed in analogous circumstances, others would experience what it had experienced itself, and that the fear of falling would hinder them from ascending. It extended, therefore, to beings who were strangers to it, an idea which was personal to itself: and from a particular circumstance it formed a general rule.

It was frequently fatigued with the numerous visits which it received, and would hide itself under its coverlid; but it never did this except when strangers were present.

My observations on the intellectual means resorted to by ourang outangs for their defence, are confined to these facts alone; but they are sufficient, in my opinion, to prove that these animals are able to make up by the resources of intellect, for their feeble corporeal organization.

On the intellectual Phenomena, which have for their Object, to procure for the Animal such Things as are necessary for its Subsistence.

The natural wants of the ourang outangs are so easily satisfied, that these animals must find in their organization, enough of resources, not to compel them to a great exertion of their intellectual faculties in this respect. Fruits are their principal food, and, as we have already seen, their limbs are peculiarly adapted for ascending trees. It is probable, therefore, that, in their state of nature, these animals employ their intelligence much oftener to preserve themselves from harm, than to procure food. But all their habits must change, the instant they are in the society, or under the protection of men: their dangers must be diminished, and their wants increased. This is evinced by all the domestic animals, and *a fortiori* by our ourang outang. In short, its intelligence was much more frequently called into action, to satisfy its wants, than to avert danger. I ought to place in this first division, a custom of this animal, which appeared to be a phenomenon of instinct, the only one of the kind which it exhibited. While the season did not admit of its leaving the house, it practised a custom which appeared singular, and which was at first difficult to account for: this consisted in mounting upon an old desk to perform the functions of nature; but as soon as the warmth of spring admitted of its going into the garden, this extraordinary custom was accounted for: it never failed to ascend a tree when it wanted to perform these functions, and this method has even been resorted to, with success, as a remedy for its habitual constipation: when it did not ascend the tree of itself, it was placed upon it; and if its efforts produced no evacuation, it was a proof that bathing was necessary.

We have already seen that one of the principal wants of our ourang outang was to live in society, and to attach itself to persons who treated it with kindness. For M. Decaen it had a particular affection, of which it gave daily proofs. One morning it entered his apartment while he was still in bed, and threw itself upon him embracing him strongly, and applying its lips to his breast, which it sucked, as it used to do his fingers. On another occasion it gave him a still stronger proof of its attachment. It was accustomed to come to him at meal times, which it knew very well, in expectation of victuals. With this view it leapt up behind his chair, and perched upon the back of it; when he gave it what he thought proper. On his arrival in Spain, M. Decaen went ashore, and another officer of the ship supplied his place at table: the ourang outang placed itself on the back of the chair as usual; but as soon as it perceived a stranger in its master's place, it refused all food, threw itself on the floor, and rolled about in great distress, frequently striking its head, and moaning bitterly. I have frequently seen it testify its impatience in this way: when any thing was refused it which it wanted, not being able or not daring to attack those who opposed its wishes, it would throw itself on the floor, strike its head, and thereby endeavour to excite interest or pity in a more lively manner. This method of expressing sorrow or anger, is not observable in any animal, man excepted. Was this ourang outang led to act in this manner from the same motives which actuate us in similar circumstances? I am inclined to answer this question in the affirmative: for in its passion it would occasionally raise its head from the ground and suspend its cries, in order to see if it had produced any effect on the people around, and if they were disposed to yield to its entreaties: when it thought there was nothing favourable in their looks or gestures, it began crying again.

This desire for marks of kindness, generally led our ourang outang to search for persons whom it knew, and to shun solitude, which seemed to displease it so much, that one day it employed its intelligence in a singular way to break loose from it. It was shut into a closet adjoining the room where the people of the house usually met: several times it ascended a chair in order to open the door, which it effected, as the chair usually stood near the door, which was fastened with a latch. In order to prevent it from repeating this operation, the chair was removed some distance from the door; but scarcely was it shut when it again opened, and the ourang outang was seen descending from the chair, which it had pushed towards the door, in order to enable it to reach the latch. Can we refuse to ascribe this action to the faculty of generalizing? It is certain that the animal had never been taught to make use of a chair for opening doors, and it had never even seen any person do so.

All that it could learn from its own experience was, that by mounting upon a chair, it could raise itself to a level with things that were higher than it; and it may have seen from the actions of others, that chairs might be moved from one place to another, and that the door in question was moved by lifting the latch: but these very ideas are generalizations, and it is only by combining them with each other, that the animal could have been led to the action which we have related. I do not think that any other animal ever carried the force of reasoning further. To conclude:—Men were not the only beings of a different species to which the ourang outang attached itself: it conceived an affection for two cats, which was sometimes attended with inconvenience: it generally kept one or other under its arm, and at other times it placed them on its head; but as in these various movements the cats were afraid of falling, they seized with their claws the skin of the ourang outang, which patiently endured the pain which it experienced. Twice or thrice indeed it attentively examined their feet, and after discovering their nails, it attempted to remove them, but with its fingers only: not being able to accomplish this object, it seemed resigned to the pain they gave it, rather than renounce the pleasure of toying with the animals. This desire of placing the cats upon its head, was displayed on a great many other occasions, and I never was able to divine the cause of it. If some small pieces of paper fell into its hands, it raised them to its head, and it did the same with ashes, earth, bones, &c.

It has already been mentioned that it took its food with its hands or mouth: it was not very expert in handling our knives and forks, and in this respect it resembled some savages whom we have heard of; but it made up for its awkwardness by its ingenuity: when the meat which was on its plate did not lie conveniently for its spoon, it gave the spoon to the person next it, in order that he might fill it, It drank very well out of a glass, which it could hold in its two hands. One day, after having put down the glass, it saw that it was likely to fall, and it instantly placed its hand at the side to which the glass inclined, and thereby saved it. Several persons were witnesses to these circumstances.

Almost all animals have occasion to protect themselves against the effects of cold, and it is probable that the ourang outangs are in this predicament in the rainy season. I am ignorant of the means resorted to by them in their state of nature, but our ourang outang almost continually kept itself covered. When on ship-board, it laid hold of every thing that came in its way; and when a sailor had lost any of his clothes, he was sure to find them in the ourang outang's bed. The care which it took to keep itself covered furnished us with an excellent proof of its intelligence, and proved not only that it could generalize its ideas, but that it had the sentiment of future wants. Its coverlid was spread every day on a piece of grass in the garden, in front of the dining-room, and every day after dinner it went straight to the garden, took its coverlid upon its shoulders, and leaped upon the shoulders of a domestic that he might carry it to bed. One day that the coverlid was not in its usual place, it searched until it found it, and then threw it over its shoulders as usual.

I have already remarked, that this animal was by far too young to exhibit any of the phenomena connected with generation, &c. I shall here terminate my observations, although I could add a great many more facts, but they would throw no additional light on the subject of our inquiries.

What has been just stated, ought to show that it is not necessary to multiply our experiments in order to obtain general and precise ideas as to the intellectual faculties of the mammiferæ. If we pick out one or two species in each genus, and examine them under the point of view which I have adopted, I am convinced that we might succeed in establishing the laws to which this faculty is subject in the whole class, and in appreciating the successive degradations which it undergoes, its connection with the senses, and the supplementary means which nature furnishes: in a word, we might lay the foundation of this interesting branch of natural history, which has been hitherto obscured by imaginary systems or obscure facts. For my part, I am happy in having had an opportunity of studying the animal which approaches most closely to man. I regard this as a point of comparison to which I shall in future refer all the other species of the mammiferæ, if circumstances admit of my continuing the inquiries, which I long ago commenced, into the intellectual characters which distinguish these species from each other.

On the Cause of the RED COLOUR *of the* BLOOD. *From the Edinburgh Medical and Surgical Journal for Jan.* 1811.

NO fact seems to be more positively ascertained, by many respectable physiologists, than that the red colour of the blood is owing to red oxide of iron: there are, however, many others of equal authority, who do not allow that the asserted fact is proved. I shall be glad if the evidence on this point be further examined, and in the meantime offer the following experiment and observation.

Experiment.—I collected a quantity of the red part of human blood, by repeated ablutions of the crassamentum, by which I separated of course, the coagulated lymph or albumen, and obtained the part desired in the form of a deposit from the water. This deposit was evaporated to dryness. It weighed 110 grains. By burning in a platina crucible, it afforded a half-fused, brown, tasteless substance, in weight two grains and a half. It was boiled in muriatic acid, which dissolved part of it. On evaporating the solution duly, it was not found styptic to the taste; on the addition of tincture of gall-nut, it became blackish; on the addition of prussiate of potash, it afforded a deep blue-coloured precipitate, which, on calculation, however, did not yield above one half of a grain of reddish-brown powder. Now, by estimation, this half a grain of reddish-brown powder was obtained from 10,000, or twenty ounces of blood. Is it then probable that this quantity of blood should derive its colour from half a grain of iron?

Observation.—On exposing the crassamentum of blood to heat to separate the water from it, as soon as it grows hot, the colour changes from black, or red, to brown or grey. Is it probable that this change from *red*, or black, to *brown*, would occur, if iron oxide was the colouring matter of blood?

INTELLIGENCE.

DOMESTIC.

College of Physicians and Surgeons.

THE following extract from the Report made by the Committee of the Regents of the University, evinces the flourishing condition of the above Institution, and proves that the most sanguine expectations may be justly entertained of its future importance and prosperity.

"The committee to whom the report of the College of Physicians and Surgeons in the city of New-York, with the papers accompanying the same, were referred—Report,

"That they view with satisfaction the advancement of the College to a greater degree of prosperity, than it has at any time heretofore enjoyed. The names of the students reported, as attending the several classes, are eighty-four in number, which is a flattering indication of the continued progress of this Medical School. The President and Professors appear to have made great exertions to promote its welfare and permanent success; and their zeal for the cultivation of medical science, merits the approbation of the Regents.

"The chemical and anatomical professorships, as they are attended with much expense to the incumbents, deserve, in the opinion of your committee, some aid and support from the Regents; and it is proposed that the annuity of five hundred dollars, lately granted by the legislature for the use of the college, should be appropriated for this necessary and important object.

"As the Vice-President of the college at present holds no professorship, and as he is willing to devote his services to the institution, by giving medical lectures, your committee are of opinion, that he should be appointed accordingly to aid in delivering the necessary courses of instruction to the students; and your committee are not advised that it will be necessary or proper to make any other appointment of professors at this time."

At the same meeting diplomas were granted to nineteen gentlemen, (a list of whom we shall publish in our next,) and the following persons were appointed trustees of the college.

Thomas Addis Emmet, Andrew Morton,
Richard Kissam, Andrew Hunt, and
Lyman Spalding, Joseph Bloodgood.

Bill of Mortality for Portsmouth, New-Hampshire, for A. D. 1811. By Lyman Spalding, M. D.

Complaint.	*Age.*	*No.*
Angina Pectoris	58· years	1
Aphtha	1. month	1
Apoplexy	75‘ years	1
Atrophy	2. m 4. 7. 68, 64, 7, 2. 3. years	8
Cholera of infants	1, 2. years	2
Colick, bilious	17. years	1
Consumption	29‘ 60· 45‘ 35‘ 20. 48, 30‘ 59‘ 25‘ 48· 40· 33‘ 68‘ 69, 25· 30· 20. 70. 32· 44· 35‘ 60· 65· 54, 81· 30‘ years	26
Convulsions	1. y 3, d 3. w 6. y 2. 3. 1, w 2. y 2. m 1. w 1. m 1. d	12
Cinanche maligna	3. years	1
Diseased vertebræ	24, years	1
Dropsy	64· 60‘ 68‘ 31‘ years	4
Dropsy in the brain	3. 2, y 2. 3, m 14. 4. 1. 1, year	8
Erythema	2. 3, months	2
Fever pulmonic	2. years	1
Hemorrhage	28‘ 35‘ 69‘ years	3
Hooping cough	4. years	1
Intoxication	31· years	1
Mortification	1, 48, 2, 90, 4, years	5
Old age	87· 83‘ 70‘ 76‘ 90, 87· 74‘ 81· 80, 88· 90· 85· years	12
Palsy	69. 78‘ 58‘ 72‘ years	4
Phrenitis	30· 8, years	2
Quinsy	2, years	1
Rheumatism	55· years	1
Scrophula	26, 10, years	2
Small-pox	20. years	1
Tetanus	14. years	1
Casualties.		
Drowned	78‘ 62· 50· 40. years	4
Falling of earth	35· years	1
Suicide	18, 41. years	2
Births.		
Males	130	
Females	124	
Still born	15	
Marriages	69	

Total 110

Portsmouth, the capital of the State of New-Hampshire, situated 43° 5′ north latitude, and 6° 26′ east longitude from Washington, contains 6934 inhabitants.

Note. This bill is so constructed as to show the complaint, age, sex, &c. and whether married or unmarried. When a period follows the age, it denotes the male sex, a comma the female sex; when in their usual place at the bottom of the line, unmarried—at the top of the line married.

FOREIGN.

Researches on the light manifested in the combustion of Inflammable Substances, by COUNT RUMFORD, *F. R. S. and Foreign Associate of the National Institute; read at the sitting of the first class of the Institute, October* 14, 1811. *Translated by* JAMES LOW, *M. D. and Secretary to the Society of Arts.*

When an inflammable substance, in a state of purity, such for example as wax, fat, or purified oils, is burnt entirely, or without leaving any residuum, and with a transparent, lively, white flame, and without smoke or odour, its combustion is regarded as complete, and its products (water in the state of vapour, and carbonic acid gas) are always identical, and in the same proportions for each substance.

Those who consider light as a particular substance emanated from luminary bodies, have been obliged to seek the source of that which is manifested in combustion, among the different substances which unite in this process. Some suppose that the inflammable substance furnishes it, others that it proceeds from the air which supports the combustion; and some have imagined that it is furnished partly by the air, and partly by the inflammable substance which is burnt.

If this light was in reality one of the chemical products of combustion, as has been supposed, we ought, without doubt, to find it *pre-existing* in these bodies, and we are authorized by every analogy, to conclude, that in this case, the quantity of light developed in the perfect combustion of a given quantity of any inflammable substance, ought necessarily to be limited, and as uniform as the other chemical products of the process.

But if light is not a substance emanated from luminous bodies, but a vibration and undulation in ether, analogous to the undulation in air, which is the immediate cause of sound, as a great number of distinguished philosophers have thought; in this case, we ought to seek the cause of the light diffused in a flame, in the very high temperature of the different particles of matter which compose this flame; these particles would be luminous in the same manner as red hot iron is luminous, and would disappear at the instant when they became so cold, that their light (or the vibrations which they excite in ether) should cease to be visible.

In adopting this hypothesis, we are not to expect to find the quantities of light bearing a constant proportion to the quantities of inflammable matter burnt; much less ought we to expect the existence of such a proportion, as a demonstrative proof of the falsity of the hypothesis, for this reason, —The volume and form of a flame ought necessarily to have a great influence on the celerity of its cooling; and if it shall be found that these circumstances have no influence on the quantity of light which is diffused, it will be sufficiently proved, that this light does not depend on the preservation of the heat of the particles which compose this flame.

But, if it shall be found by the result of decisive experiments, that the light diffused in the perfect combustion of the same quantity of inflammable matter is variable; it will be impossible, it appears to me, not to perceive that this light cannot be a chemical product of combustion, and that the hypothesis which supposes light a substance emanated from luminous bodies, would become more and more difficult to maintain.

If this was the agitation of a question merely speculative, and which in its nature could have no real influence either on the progress of the sciences, or on the useful arts which depend on them, I should be the first to condemn its discussion, not only on account of its inutility, but also, and particularly, on account of the disagreeable consequences to society, which always result from tedious and endless disputes.

To perceive all the importance of the question we have agitated, we have only to consider the great advantage to the public, which cannot fail to result from the discovery of a fixed principle, which may be employed with facility and success, to perfect the art of *illumination,* and the instruments employed in it.

What enormous sums are annually and daily expended to dissipate the darkness of night! and, moreover, in what a deplorable state do we find the science which ought to illustrate all the details of this important operation!

How shall we attempt to perfect the art of *illumination* so long as we are so ignorant of the nature of light, that we do not even know where to go in search of it?

Having considered this interesting subject for a long time, I have made a number of experiments which appear to me calculated to throw light on it. I am about to submit them to the class, but it will be necessary to commence by premising a few words on a small change which I made on an apparatus already known, and of which I availed myself in measuring the intensity of light.

Instead of employing the rules divided into inches and tenths of inches, to measure the distances of lights on the photometer, I used rules divided into degrees, which indicated directly, and without any calculation, the relative intensities of the two lights which are compared together.

These two rules, which are each twelve feet long, are divided in a similar manner. The first division of this kind of *scale of light,* which is on one of the extremities of the rules, is marked 10 degrees, and corresponds to ten inches of the middle of the vertical bottom of the photometer, when the rule is adjusted. The division which is marked 100 degrees, is at the distance of 21.62 inches from the first, (that of 10°) or at 31.62 inches from the centre of the photometer, and all the other divisions are arranged in such a manner, that the numbers which they bear, are every where proportional to the squares of the distances of the places where these divisions are marked, from the middle to the bottom of the photometer, where the two shades which are compared coincide in the experiments.

To complete the important part of a standard of light, it is necessary to have a very steady light, and, at the same time, one known to every one. I found these two qualities in a good candle. That which I chose, was of those which are five to a pound; it was 9 lines in diameter, 10 inches and a quarter long, and weighed 90 grammes, (about 3 ounces); it burnt 7 grammes of wax per hour, with a beautiful flame, and with an uniformity so perfect, that it rarely varied a fortieth in its ordinary consumption.

I have given to the light of this candle the value of 100 degrees, and I placed the candle always at the division of the scale of the photometer, which is marked 100 degrees, when it is burning. In order to express in a uniform and convenient manner the quantities of the different inflammable substances consumed in the experiments, I divided into a 100 parts the quantity of wax which the candle I used as a standard consumed in an hour (=7 grammes), and I established one of these parts ($=\frac{7}{100}$ de grammes or 1.34545 grains) as an unit of weight (unitè de poids), and I employ it as such in all my experiments.

We shall see in the sequel, how this arrangement has rendered the comparison of the results of my experiments easy and intelligible.

The object I had particularly in view in the course of experiments, of which I now have the honour of giving an account to the class, was to datermine if the quantity of light manifested during the combustion of an inflammable substance, is always in an invariable proportion to the quantity of the substance which is consumed; and as the flame of an Argand's lamp has a double current of air, well regulated, and is very beautiful; and as the purified oil, employed in these lamps in Paris, burns without smoke or odour, and without residuum, I endeavoured, moreover, to discover whether this lamp always gave a quantity of light which was in an invariable proportion to the oil burnt. The following are the experiments and their results.

Exp. 1.—An excellent lamp, which had been carefully cleaned, adjusted, and weighed, was lighted and placed before the photometer, where it was regulated, for 30 minutes, in such a manner as to afford constantly 100° of light; the same quantity which the candle had furnished.

At the end of this experiment, the lamp was extinguished, and on weighing it anew, I found there had been 8 grammes of oil consumed.

This quantity is=114 parts, consequently the consumption of the lamp was in the proportion of 228 parts of oil per hour, for 100° of light; or for 100 parts of oil, it had yielded 48° of light.

The candle consumed 100 parts of wax, and gave 100° of light.

Exp. 2.—The lamp having been cleaned, adjusted, and weighed again, was placed before the photometer, directly opposite the division of its scale marked 200°, when, after having been lighted, it was regulated in such a manner, for 30 minutes, as to give uniformly 200° of light.

In this experiment, 10.3 grammes of oil were consumed.

That is, in the ratio of 271 parts of oil per hour, for 200° of light, or for 100 parts of oil, 74° of light.

Exp. 3.—The lamp having been cleaned and adjusted as before, was placed for the third time before the photometer. In this experiment, it gave during 30 minutes, 300° of light, and consumed 10.7 grammes of oil.

That is, in the ratio of 305 parts of oil per hour, for 300°; or for 100 parts of oil, 98° of light.

Exp. 4.—The lamp having been carefully prepared and placed before the photometer, was managed for thirty minutes in such a manner as to give constantly 400° of light, and it burnt 12.7 grammes of oil; that is, in the ratio of 361 parts of oil per hour, for 400° of light; or for 100 parts of oil, 112° of light.

This is the first experiment where a given quantity of oil has furnished more light than an equal given quantity of wax; but without stopping to make any observations on this fact, I pass on to more important results, and to save time, I shall exhibit in a tabular form, the results of the four experiments, of which I have given some account, and shall add to them the results of five others, which as well as the four preceding, were all made the same day, with the same lamp, the same candle, and the same care.

Order of Experiments.	Intensity of the light furnished by the lamp during 30 minutes.	Quantity of oil consumed per hour.	Light furnished per hour, from the consumption of 100 parts of oil.
1	100°	228 parts	48°
2	200°	271	74°
3	300°	305	98°
4	400°	361	112°
5	500°	405	121°
6	600°	441	138°
7	700°	470	149°
8	800°	515	155°
9	900°	560	160°

On comparing the results of these nine experiments, we find that the quantities of light furnished, were far from being in a constant proportion to the quantities of oil consumed, as should have been the case, if light was one of the chemical products of combustion. The quantity of light afforded by 100 parts of oil, was almost four times as much in the ninth experiment as in the first, though the flame was equally clear in both, as indeed it was in all the other experiments, and had neither manifested smoke or odour.

As it appeared very probable to me, that a small flame of a particular form, always yielded less light in proportion to the oil consumed, than a larger one of the same form; to illucidate this fact, I made the four following experiments.

I had a small portable lamp made, with a wick composed of four flat wicks, touching each other in the centre of a circle, in such a manner, as that a horizontal section of this compound wick, formed a cross at right angles. Each of these four flat wicks was three lines broad; and in adjusting the lamp for the first experiment, I cut each of these flat wicks in a slope, of such a degree as to leave the centre about a line higher than the circumference, and the four wicks only touched each other in the centre.

These wicks being cut in this manner, it was easy to cause the lamp to burn with a very small flame, which was necessary to answer my intentions.

I made four experiments with this lamp, and with the following results:

Order of Experiments.	Intensity of the light furnished by the lamp.	Quantity of oil consumed per hour.	Light furnished per hour, from the consumption of 100 parts of oil.
10	25°	67 parts.	37°
11	100°	143	70°
12	225°	211	112°
13	255°	214	118°

In the thirteenth experiment there was three times more light from a given quantity of oil than in the tenth.

I made many other experiments, similar to these, with other lamps, and always with results analogous to those which we have seen; but without stopping I pass on to other experiments made with candles, which have afforded the most interesting results.

Exp. 14.—To determine whether the delicate flame of a small candle gave less light than the flame of an ordinary candle, in proportion to the wax which it consumed, I made the following experiment:

A small candle of the kind used in dark lanthorns, four lines and a half in diameter, was placed before the photometer, when it burnt sixty minutes, giving constantly 64° of light.

During this experiment, it consumed 2.7 grammes of wax, that is, 77 parts of wax per hour, for 64° of light. Wherefore, from 100 parts of wax, we should only have had 83° of light instead of 100°, which an ordinary candle gives from the same consumption of wax.

Exp. 15.—A small watch lamp, seven lines in diameter, and two inches long, and which weighed 11.15 grammes, was placed standing and floating in a small vessel, in the manner that this kind of watch lamp is always arranged. After having burnt tranquilly for two hours and forty minutes, it was extinguished, wiped, and then weighed anew.

It was found that it had consumed 4.75 grammes of wax, which gave 1.7812 grammes,=25 parts of wax, for the consumption of this small taper per hour.

If it had yielded as much light in proportion to this consumption as an ordinary candle, we should have found its light=25°.

Having measured its light by means of the photometer, I found it=1.52°, or a little more than a degree and a half, instead of being 25°.

Here then was a flame, and even the flame of a candle, which was sixteen times more faint than it ought to have been from the quantity of wax which disappeared.

Notwithstanding the results of the preceding experiments, and the reflections to which they gave rise, had prepared me to find the light of this small watch lamp very feeble, I was greatly surprised at finding it so extremely faint. I immediately repeated the experiment several times, and with the greatest care, and notwithstanding the little candle gave sometimes for an instant a little more light, it as often gave only half the quantity above stated, and I am persuaded that in estimating its light at a degree and a half, we give it its maximum.

This result can be satisfactorily explained, admitting the hypothesis which regards light as being analogous to sound. The flame of the watch lamp was very small, the particles which composed it were cooled so rapidly, that they had scarcely time to shine an instant before they disappeared entirely.

It would have been extremely natural to have attributed the faintness of the light in this experiment to the feebleness of the combustion, if the quantity of wax consumed had not afforded a demonstrative proof that the combustion was not in the least faint.

But here a very interesting question presents itself: if it has been proved to a certainty, from the quantity of wax burnt, that the process of combustion took place with its ordinary vivacity, what became of the heat which ought to have been developed in the operation?

I have sought it, and have had the satisfaction not only to discover it, but to discover it entirely.

I found that the small candle never ceased to furnish it for an instant during the whole course of the experiment.

A moment's reflection made it obvious that the heat which the particles composing the small flame parted with so rapidly, could not be lost entirely, and that it would be found in the current of warm air which rose above the flame.

Holding my hand open at the distance of two or three inches above the faint flame of the little candle, I found in reality a degree of heat that astonished me, and was altogether insupportable, so that I was obliged to withdraw my hand suddenly.

I believe that a burn never gave any person so much pleasure as that did me.

I lost no time in prosecuting an experiment which should clear up this mystery in such a manner as not to leave any doubt.

Exp. 16.—I had fortunately in my laboratory an apparatus ready prepared, (I had used it in other experiments) which was well calculated for the experiment I was desirous of making. This apparatus consisted of a small cauldron of tinned iron, well polished on the outside, and of a conical form, with a circular bottom, seven inches and ten lines in diameter, and a little swelled inwardly. This cauldron was four and a half inches deep, and six inches wide at the top, and tapered off above like a wine bottle, having a long neck, in which was placed a mercurial thermometer, which served to indicate the temperature of the liquid which the cauldron contained. I placed this little cauldron upon a small board, an inch thick, which had a hole in its centre, about three inches in diameter. This board was attached to an upright post, in such a manner that it could be fixed at any desirable height; the lamp was placed below it on a table.

Having put into this small cauldron 2000 grammes of water, at the temperature of 65° Fahrenheit (which was also the temperature of the room in which the experiment was made) a small candle of a known weight placed in its vessel, and floating on the water where it was placed, was lighted, and put afterwards under the cauldron, at such a distance that the point of the small flame reached on a line with the inferior surface of the board on which the cauldron was placed, and below the centre of the hole made in the board, to give passage to the flame.

The following is the result of this experiment: The small candle having burnt under the cauldron for fifty-two minutes and fifteen seconds, the thermometer indicated the water to have acquired the temperature of 75° Fahrenheit, the candle was extinguished, and having been wiped and weighed, it was found to have lost during this experiment 1.55 grammes of wax.

Exp. 17.—The cauldron having been emptied and cooled, was filled again with 2000 grammes of water, at the temperature of 65° Fahrenheit, and placed on the board.

A good candle of the ordinary size, weighing three ounces, properly placed under the cauldron, was lighted. It had burnt tranquilly and equally for twelve minutes and thirty seconds, when the thermometer placed in the cauldron showed that the water had acquired the temperature of 75° Fahrenheit. I immediately extinguished the candle, and on weighing it anew, I found that it had consumed 1.62 grammes of wax during the experiment.

The difference between the quantities of wax burnt in these two experiments, in giving the same quantity of heat to the same quantity of water, is very small (=0.07 of a gramme) and it can be readily explained, in a satisfactory manner, without having recourse to the supposition (otherwise very improbable) that the temperature at which the decomposition of an inflammable substance takes place in combustion, is perhaps variable.

The light which is manifested in this process is to a certainty variable, and to a surprising degree.

The results of the experiments which we have related are very interesting, and the more we examine them, the more important do the facts which they present to us appear.

They teach us more fully to comprehend *light* and *heat*, and to distinguish and appreciate their effects.

There will be less difficulty in conceiving the existence of a true combustion, accompanied by its peculiar temperature, in all the cases, when the products of a chemical operation indicate it, even though the light which accompanies it should not be visible, and though the heat *found* which results from it should not evince directly the existence of so high a temperature.

There will also be less difficulty in admitting that all chemical operations have always a regard to fixed and invariable temperatures in all cases.

Since we have regarded light as a peculiar substance, we have naturally endeavoured to comprehend its combinations with other substances, and the laws of its affinities; and without permitting ourselves in the least to doubt the reality of its existence as a substance, we have become so familiarized with the idea of its accumulation, condensation, fixation, liberation, &c. that in the end this doctrine appears to contain nothing but what is simple and easy of comprehension.

If the light which shines on us was in reality a substance which luminous bodies sent to us, we might hope to be able to discover how and where it existed before it was transmitted to us; but if it is only an impulse (coup) given to the eye, by the repercussion of an elastic fluid which touches the eye, and at the same time all the bodies in the universe, we should lose time either in seeking its *source* or its chemical properties.

No person I think ever advised us to search for sound in *fulminating powder;* is it more reasonable to seek in the substance which is burnt for the light which it emits?

The light which stares us in the face only exists in the eye.

The eye is formed in such a manner as only to be sensible to impressions of a limited force, and to vibrations of a certain velocity; all others are without effect on this organ.

When a common ball is heated red hot and set in a dark place to cool, it ceases to be visible to the human eye as soon as it is cooled down to the 900° of Fahrenheit's thermometer; but can it be believed that the ball experiences a sudden and notable change in its state at the instant it becomes invisible? This supposition is unreasonable.

An inhabitant of another planet, that for example discovered by Herschell, if he had eyes differently constructed from ours, and more sensible to a small degree of light, looking with the man of this globe at the ball as it was cooling, would be greatly astonished to hear him say, suddenly, "the ball has ceased to be luminous."

It is possible, and even propable that there are animals on our own globe who perceive bodies shine after they cease to be luminous to us, and perhaps some that we are never able to see.

But if we are in a state to perceive only a part, an *unknown fraction* of that light, which luminous bodies shed; how can it be conceived that we can in any case measure it completely, and compare it with heat or any other thing?

All that we can do is, to compare the intensity of one light visible to us, with the intensity of another equally visible, in order to determine their relative power, to illuminate the objects around us; and what interests us particularly in this research, is doubtless to discover the means of arranging matters in such a manner, that the substances which afford light in their combustion shall furnish that which is useful to us in the greatest possible abundance.

It is in the application of our discoveries to perfect the art of illumination, that their importance will appear with the greatest eclat.

We have sought a long time the means of increasing the light of lamps in order to render them serviceable in light-houses, or coasts, harbours, and for other purposes, but without success. We have used lamps with a double current of air, of large dimensions, but we found that they gave less light than the same kind of lamps of the ordinary dimensions.

I believe I was the first who united together a number of lamp sockets with a double current of air in the centre of large balloons of gauze (de grands balons de gaze) for the purpose of illuminating large apartments. This invention has been found useful; but if I am not mistaken it will soon give place to one much more simple, economical, and which ought to produce a more beautiful effect.

Since I have better understood the nature of light, I have discovered the means of increasing its intensity in a focus almost at pleasure. I have just finished a lamp of a construction extremely simple, which, with four flat wicks, placed one by the side of the other at the distance of about two lines, separated in the wick-holes, to give passage to the air, affords *six times more light* than an ordinary lamp with a double current of air, or an Argand's lamp. I have often measured the intensity of its light, by means of my photometer, and have found it from 3800° to 4000°; therefore it equalled the light of 38 or 40 good candles, burning all together, and with their greatest brilliancy.

M. Professor Pictet, inspector general of the imperial university, and M. Micheli, of Geneva, assisted in an experiment made at my house, at Antenil, some days ago, when this lamp afforded us more than 3800° of light. And M. Guy Lussac, member of the institute, was present at another experiment which gave 4000° of light. On this last occasion we compared the flame of this new lamp with that of an Argand's lamp, in order to see which was the most beautiful. The two lamps having been placed near each other, we were struck with the result of the comparison. The flame of the Argand lamp appeared as yellow and dull at the side of the new lamp, as that of a candle by the side of an Argand lamp.

The most important circumstance in this invention is, that we can give to these new polyflame lamps any degree or power of illumination we wish, and can be certain of having more light from a given quantity of oil, than from any lamp hitherto known.

The fundamental principle on which this lamp is constructed is so obvious, that it requires only to be mentioned to understand how to put it in practice in every instance.

We have only to preserve the heat of the flame as much or as long as possible. How is it obvious that a very simple contrivance will accomplish this? It is by placing a number of flat wicks close to each other (large or small), in order that, uniting together, they may cover each other, and mutually defend themselves from the surrounding cold bodies.

A flame being perfectly permeable to another flame which crosses it, as I have shown already, there is no danger that a flame in covering another, will diminish the brilliancy of the light which it sheds. I have made the light of a flame pass through eight other flames, without its intensity or its power of illumination being sensibly diminished.

I ought to mention that my new lamp has no need of the narrow chimney or glass tube to quicken or brighten its four flames. It will be sufficient to place over it a large glass muffle, five or six inches in diameter, or more, and seven or eight inches high. This muffle is placed on a sheet of tin, which has a hole in the centre to admit the air, which should always come from below, to support the combustion of the oil.

I propose to publish a detailed description of this lamp as soon as I have brought it to perfection: in the mean time I have given a precise idea of the principles on which it is constructed, for the information of those who are disposed to assist me in completing it.

Case of the good Effects of Stramonium in Asthma.

Mr. J. C. a medical gentleman, 42 years of age, of middle stature and full habit, had been afflicted with a cough and difficulty of breathing for several years. About four years ago he was attacked with distinct paroxysms of asthma, which came on in the usual manner, and progressively increased, so much so that he could not lie down without the greatest dread of suffocation. This fit continued three or four weeks and then left him. In a few months it again attacked him, and for three years afterwards he had regular paroxysms, with great difficulty of respiration in the intervals. During this time he took various medicines suggested both by himself and numerous physicians of eminence, and in short, during three years, made an adequate trial of every article in the Materia Medica recommended for this complaint, but without deriving the least benefit. About 12 months ago, during a violent paroxysm, he commenced smoking the stramonium, and after using one pipe-full found the symptoms wonderfully relieved, and by repeating it once or twice a day, the paroxysm entirely subsided. He is now occasionally attacked in the night, but by rising and smoking one pipe of the stramonium the difficulty of breathing generally ceases immediately, and when this is not the case, he is so much relieved that he can lie down and sleep with comparative comfort. Since he first had recourse to this remedy, he has had no regular fit of asthma, and his breathing gets so much better, that he is of opinion that by perseverance in the remedy he shall entirely recover. It does not affect his stomach nor his head, but seems entirely to act on the organs of respiration. *Lond. Med. Rev.*

Case of Hydrophobia.

A child, aged four years, was bitten by a dog on the 10th of March, 1811. The wound, which was situated at the inner angle of the eye-lid, was very slight, and soon healed, without affording any subsequent inconvenience. On the 29th of the same month she complained of a tooth-ache, to which she had been liable. She passed a restless night with frequent sighings, and on the following morning was attacked with a convulsive action of the throat, and great agitation of her whole frame: the symptoms increased; she became feverish, and the sight of fluids produced the greatest disgust. These symptoms continued during the second and third days, without much increase: the skin was hot and dry, the pulse very frequent, and no evacuation had taken place from the bowels for a week. The difficulty of respiration, with great anxiety and sighings, were excited by the presence of liquids, or a stream of air. In the evening of the third day, *two* scruples of calomel and three grains of opium, suspended in a little mucilage, were injected into the stomach through an elastic gum tube; an aloetic clyster was also administered. This was attended with a temporary aggravation of the spasms, which afterwards subsided to their former state. During the evening she repeatedly asked for liquids, which, when offered, excited the greatest horror; the eyes became dull and heavy, with an appearance of inflammation, the paroxysms became more frequent and violent, the extremities more cold, and she was insensible: the approach of a candle excited convulsions; she spit out a quantity of mucus, and expired early on the morning of the fourth day. Upon dissection no morbid appearances were discovered, except an inflammation of the trachea and bronchia, which contained a quantity of puriform fluid. *Ib.*

DEATH.

In New-York, on the 17th of March, 1812, Edward Miller, M. D. Resident Physician of the Port of New-York, and one of the Editors of the Medical Repository.

END OF VOL. III.

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